KNOWLEDGE
AN ILLUSTRATED
MAGAZINE OF SCIENCE
PLAINLY WORDED—EXACTLY DESCRIBED

*LONDON: FRIDAY, NOVEMBER* 4, 1881.

CONTENTS.

| | PAGE. |
|---|---|
| To Our Readers | 3 |
| Science and Religion. By the Editor | 3 |
| The Relation of Food to Muscular Work.—Part I. By Dr. W. B. Carpenter, F.R.S. | 5 |
| Are Women Inferior to Men? | 6 |
| Comets.—(*Illustrated*) | 8 |
| Illusions. By Thomas Foster—(*Illustrated*) | 10 |
| The Eastern Skies in November—(*Illustrated*) | 13 |
| Darwin on Mould and Worms | 14 |
| Correspondence.—Introductory—Hints to Correspondents—Is the Sun Hot?—Can Ice-Yachts sail faster than the Wind? | 15 |
| The Birmingham and Midland Institute. By W. Mattieu Williams | 16 |
| Colour of Sunlight | 18 |
| The *Tribune* Riddle | 18 |
| Our Mathematical Column | 19 |
| Our Chess Column | 19 |

## TO OUR READERS.

"KNOWLEDGE" is a weekly magazine intended to bring the truths, discoveries, and inventions of Science before the public in simple but correct terms—to be, in fact, the minister and interpreter of Science for those who have not time to master technicalities (whether of Science generally or of special departments).

Knowledge will contain *Original Articles* by the ablest exponents of Science; *Serial Papers* explaining scientific methods and principles; *Scientific News* translated into the language of ordinary life; a *Correspondence Section* (including columns of *Notes and Queries*) for free and full discussion, and especially for inquiry into matters which the readers of original articles may find difficult or perplexing; and *Reviews* of all scientific treatises suitable for general reading. In addition to these portions, there will be a section for Mathematics, and columns for Chess and Whist (regarded as scientific games), conducted on a novel plan intended to render these portions at once useful to learners and interesting to proficients.

I have long regarded the material benefits derived from Science, great though these unquestionably are, as small by comparison with those to be derived from Science as a means of mental and moral culture. Nearly ten years have passed since, recognising this, I pointed out the necessity for such a journal as the present. We have none doing the work which Knowledge is intended to perform.

But I felt that before attempting to conduct such a Journal, I should obtain as wide an experience as possible of the wants of the class of readers for which it is intended. During the last ten years I have come in contact, as lecturer and writer, with tens of thousands belonging to that class. The experience I have thus gained is altogether exceptional. From letters addressed to me during my lecture-tours in Great Britain, the United States, Canada, Australia, Tasmania, and New Zealand, as well as those almost equally numerous written to me from other countries, I have learned the nature of the difficulties which commonly perplex scientific students and the readers of scientific treatises. I believe this experience must prove of great value to me in conducting Knowledge.

The tone of the articles will be that which I have found most useful in lecturing and writing. The general public do not want Science to be presented to them as if they were of intelligence inferior to their teacher's. But they cannot be expected to take interest in statements couched in abstruse or technical terms. Nor is Science degraded when plain untechnical language, such as we propose to use in Knowledge, is employed; when, for instance, instead of speaking about the "mean equatorial horizontal solar parallax," a writer refers to the sun's distance; or when a race of men is not described as "microseme and dolichocephalic," but as small-faced and long-headed.

Discoveries and inventions communicated to scientific societies at home and abroad will not be presented until they have been translated from technical language which to the general reader is mere jargon.

The price of Knowledge is lower than that which has heretofore been assigned to periodicals of the same class. It is trusted that those who approve of the plan above sketched, and wish to see the Magazine firmly established, will help to

"Let knowledge grow from more to more,"

by making its existence known to their friends. In this and other matters cordial co-operation from all quarters is invited.

Richard A. Proctor.

## SCIENCE AND RELIGION.

By the Editor.

MANY seem to imagine that the tendency of Science, especially in its more recent developments, is irreligious. Some give a special reason for this strange opinion, namely, the inconsistency which they conceive to exist between some of the results to which modern Science unmistakably points, and ideas which have been derived from poetical descriptions found in the Bible. So far as this particular form of objection is concerned, Science need be at no pains to formulate a reply. It would be as reasonable to do so, I conceive, as it would be seriously to answer such a question as this: "How can the Darwinian theory of the remote cousinship of man and monkeys be reconciled with Job's statement (Job, xxx. 29), 'I am a brother to dragons'?" or this: "How can the views of modern medical men about the intestines be reconciled with Job's assertion (Job, xxx. 27), 'My bowels boiled and rested not'?" Moreover, the world is not interested (or should not be) in hearing the views of Science as to the real meaning of words which theologians, after much time and trouble given to a matter lying

specially within their province, are not at one in interpreting. But when the question is of the truth of those scientific views which are oppugned, or as to the bearing of Science generally on Religion, the case is different. Science may reasonably answer questions relating to the influence of scientific discoveries on the human mind.

The great objection raised against modern science appears to be in the main this, that it enlarges unduly our ideas of the vastness of God's domain in space, of the immensity of the time periods during which He acts, and in fine, of His inconceivable power and wisdom. We may admire the wisdom of the Almighty, as shown in the pebble, or the rock, in the flower or in the tree, in the insect or the animal, nay, we may even so far extend our vision as to recognise the laws under which a stratum, or a forest, or a race of animals, perhaps even a continent, or a flora, or a fauna, had their origin and passed through their various stages of development. But we must not extend our survey further. To see God's hand in these, His wisdom in the laws by which they are formed, is to be religious and good, but to trace His power and wisdom on a larger scale is to be irreligious and wicked. Evolution on the small scale we may admit without harm; but to see evolution in the dev lopment of a world or a world-system, and still more toesee evolution throughout the entire universe as revealed to man, this is "to set God on one side in the name of Universal Evolution."

It is unfortunate that those who take this view of the general scope of modern scientific research had not been careful at an earlier date to explain, when admitting the growth of a tree, a forest, or a flora—of an animal, a race, or a fauna, according to natural laws, and even explaining (as many of them did) the wonderful nature of the laws according to which such growths took place, that they wished it to be clearly understood that in thus recognising the action of law they were rejecting the idea that the Almighty fashioned the plant or the animal, the forest or the race, the flora or the fauna, or indeed, aught (animate or inanimate) the development of which man is able to study through all its stages. Because, if it had been clearly understood that wherever they recognised growth and development as the results of law, they were assured such results could not possibly be attributed to the Almighty, Science might perhaps, (though it seems unlikely) have been deterred from researches leading to the distressing conclusion that there is development according to law on the greater scale as well as on the less; nay, that to all appearance law prevails throughout the entire domain of the Almighty in space and during the entire period of time in which he acts—that is, throughout infinity of space, and during eternity of time.

As regards the actual evidence of the vastness of space and the immensity of time throughout which the action of law extends, it may suffice to say that only the very ignorant or the very dull can for a moment entertain doubt. Unless the evidence given by earth and heaven has been specially devised to mislead man, or unless the reasoning powers bestowed on man by God have been given but to lead him astray (conceptions alike blasphemous and unreasonable), there can be no manner of doubt that on the one hand the universe is infinitely larger than it was supposed to be before the days of Copernicus and Kepler, Galileo and Newton; or that, on the other hand, our earth has lasted, and will last, thousands of times as long as had been supposed before its structure had been examined; the solar system millions of times as long as had been supposed before its movements had been studied; the galaxy of stars yet longer; the higher order of systems to which that galaxy belongs for periods so vast, that to all intents and purposes they extend (in our conception) to absolute eternity—in the past as in the future.

As to the influence which a result such as this should have upon men's minds, it should perhaps suffice to say that those who believe that the Almighty is all-wise as well as all-powerful ought not to fear lest the discovery of truth from the study of His universe should produce evil effects.

But I go much further than this, and say that of all possible forms of teaching, those derived from or based upon science must be most beneficial in the religious sense, not using the words science and religion in their ordinary narrow significance, but in their widest and noblest. "Doubtless," as Herbert Spencer has well said, "science is antagonistic to the superstitions that pass under the name of religion; but not to the essential religion which these superstitions merely hide. Doubtless, too, in much of the science that is current there is a pervading spirit of irreligion; but not in the true science which has passed beyond the superficial into the profound." Or, as Huxley has even more pointedly remarked, "Trüe Science and true Religion are twin-sisters, and the separation of either from the other is sure to prove the death of both. Science prospers exactly in proportion as it is religious, and religion flourishes in exact proportion to the scientific depth and firmness of its basis. The great deeds of philosophers have been less the fruit of their intellect than of the direction of that intellect by an eminently religious tone of mind. Truth has yielded herself rather to their patience, their single-heartedness, and their self-denial, than to their logical acumen." To which may be added the noble saying of Carlyle, that "to know the Divine laws and harmonies of this universe must always be the highest glory of a man, and not to know them the greatest disgrace for a man."

But we may fairly go even further than this. We need not be content to defend, or merely to justify, or even to laud, Science in its relation to Religion. We may assert without fear of valid contradiction that the neglect of science is irreligious. For what is such neglect (where men have time and leisure for the work) but the refusal to study the works of the Creator? And in what position, logically, does a man stand who praises the Creator in words, but declines to study His creation? "Suppose," says Spencer, "a writer were daily saluted with praises couched in superlative language. Suppose the wisdom, the grandeur, the beauty of his works were the constant topics of the eulogies addressed to him. Suppose those who unceasingly uttered these eulogies on his works were content with looking at the outside of them, and had never opened them, much less tried to understand them. What value should we put upon their praises? What should we think of their sincerity? Yet, comparing small things to great, such is the conduct of mankind in general in reference to the universe and its cause."

The study of science implies the surest belief that God's works are worth study, the fullest recognition that the author of those works is worthy of our reverence. It is the truest kind of homage, in that it is not homage expressed merely in words, but homage shown in work, in service, in sacrifice. The man of science, in fine, refuses to offer to the Almighty "the unclean sacrifice of a lie." He offers him instead (in the search for truth) the sacrifice of time, of labour, and of thought. His very questions imply the fulness of his faith :—

> *This* is his homage to the mightier powers,
> To ask his boldest question, undismayed
> By muttered threats that some hysteric sense
> Of wrong or insult will convulse the throne
> Where Wisdom reigns supreme.

# THE RELATION OF FOOD TO MUSCULAR WORK.

By Dr. W. B. Carpenter, F.R.S.

[In an article on the "Use and Abuse of Food," republished in my "Pleasant Ways in Science," there is a passage in which Liebig's mistake about the relation between nitrogenous or flesh-forming food and work is quoted without being corrected. I was not aware when the article was written (1867) that scientific experiments were in progress which were eventually to completely expose the fallacy of Liebig's position. When the article was republished these experiments had long since been brought to a satisfactory issue. Although the point does not importantly affect my essay regarded as a whole (for the material of a machine, as well as the source of its working energy must be kept in repair, and the workman does not get less good from his food because he takes it under a misapprehension as to the particular benefit it will do him), the mistake is one which should have been corrected. The interpreter of scientific statements, too technical for general comprehension, must not be content with presenting correctly and intelligibly the accepted teaching of an authority in any special branch of science. He must assure himself, as time passes, that the teaching which was regarded as sound when the subject was first dealt with, has not undergone correction in the mean time. If I had done this in the present case (as I have in general been careful to do), the error in question would not have appeared in the pages of "Pleasant Ways in Science." The following exceedingly interesting paper, by Dr. Carpenter, puts the matter in the proper light. He speaks, I need hardly say, "as one having authority." I may point out that it is one of the great advantages of a journal like the present that errors into which even the most careful will fall from time to time, will here be corrected at once. In the columns of a monthly magazine correspondence would be inconvenient, even if permitted. Here the freest questioning and discussion is invited, and it is particularly desired that those having special knowledge of a subject will call attention to, and correct, any statements which may appear to them erroneous.—Ed.]

## PART I.

THAT "the evil which men do lives after them," is often exemplified by the continued prevalence of scientific doctrines accredited by the authority of great names, long after their fallacy has been demonstrated by the subsequent researches of other inquirers to the satisfaction of all competent judges. For, if these demonstrations be not presented to the world under the sanction of a like authority, the old errors are continually reproduced by popular expositors, and unquestioningly accepted by ordinary readers.

Having met with a notable instance of this kind not long ago, in the reproduction, as an accepted physiological verity, of the doctrine of Liebig as to the direct dependence of muscular energy on the expenditure of nitrogenous food, I think that a journal which aims to communicate positive "knowledge" to its readers may be an appropriate medium for a brief statement of what are now accepted by all scientific Physiologists as the facts of the case.

It is no derogation to the well-established fame of Liebig as one of the greatest Chemists of his day,* to affirm that when he passed out of his own domain into that of Biology, he made many and flagrant mistakes. Looking back after an interval of nearly forty years, at his "Organic Chemistry in its Relations to Physiology and Pathology," I am really astonished at the reckless audacity of some of his assertions; as, for instance (1st edit. 1842, p. 219), that "we know with certainty that the nerves are the conductors of mechanical effects, and that by means of them motion is propagated in all directions;" and that "the heart and intestines do not generate the moving power in themselves, but receive it from other quarters." He might just as well have said that "we know with certainty that when a charge of gunpowder or dynamite is exploded by an electric spark, it is the conducting wire that supplies the energy which rends asunder the rock." For nothing was even then more certain, than that the heart, intestines, and all other muscles furnish, in virtue of their own contractility, the power (or, as it would now be called, the "potential energy") which produces their mechanical effects, this being simply called into action by the nervous stimulus.

Another most noteworthy example presents itself in Liebig's denunciation of the "germ-doctrine" of fermentation and contagion, which was then being built-up on the basis supplied by the microscopic discoveries of Cagniard de la Tour on the fungoid nature of yeast, and of Audouin and Milne-Edwards on the like character of the Muscardine-disease of silkworms. "A theory," he says (3rd edit. 1846, p. 212), "of the cause of fermentation and putrefaction, which is utterly fallacious in its fundamental principles, has hitherto furnished the chief support of the parasitic theory of contagion. The advocates of this theory regard putrefaction as a decomposition of organic beings caused by infusoria and fungi, and consider every putrefying body as a breeding-place for infusoria or a nursery for fungi; and where organic bodies putrefy over a large surface, the whole atmosphere, according to this view, must be filled with the germs of these infusoria and fungi. The germs of these organised beings are, in this theory, the germs of disease or of the causes of disease." Yet it is this very doctrine, the complete establishment of which by the admirable researches of Pasteur, and of those who have followed in the line of inquiry which he so clearly marked out, has not only given the true interpretation of the phenomena of Fermentation, but, in its application to Pathology, is now serving as the basis for "preventive" medicine and "antiseptic" surgery. And I feel sure that Liebig himself, had he lived into the present era, would have been quite ready to admit its truth; for he was the last man to persist in views no longer tenable, merely because he had himself advanced them. "There is no harm in a man's making mistakes," he used to say, "but great harm in his *making none*, for he is sure not to have worked." And Professor Hofmann records having been exhorted by Liebig "not to keep in your house from night till morning an error you have become cognisant of."

Now Liebig's chemical division of food-materials into plastic, or "tissue-forming," and respiratory, or "heat-producing," was unquestionably an immense advance; and the basis of it is still universally recognised as sound. I can myself remember the time when it was a question whether the nitrogenous components of the blood, and the tissues formed at their expense, can be built-up in the animal body, with the aid of atmospheric nitrogen, out of starch, sugar, fat, and other non-nitrogenous hydrocarbons. Chemistry had not then shown the almost exact conformity of many Vegetable compounds to the albumen which was regarded as the fundamental constituent of Animal food; and while it was by Mulder that the doctrine of the *protein*-compounds was worked out, which, with some modifications, is now generally accepted, it was by Liebig that the impossibility of supposing that animals can form their tissues out of anything else than the "proteids" originally generated by Plants, was first definitely insisted on. He was not aware, however, that the formation of "protoplasm" requires *fat* as well as proteids; and that thus fat is to some extent a tissue-food. But he was unquestionably right in affirming that animal tissues cannot be manufactured out of *saccharine* compounds (as starch and sugar), unless these have been previously

* See "The Life-Work of Liebig," by Prof. Hofmann, the "Faraday Lecturer" for 1875.

XUM

changed into fat—a change whose possibility he went far to prove,—and then only in the same measure as fat itself. And he quite correctly assigned as the ultimate destination of the great bulk of the non-nitrogenous components of food, the being (so to speak) "burned" within the body, by combination with oxygen taken in through the lungs; their hydro-carbons being exhaled by these organs in the form of water and carbonic acid. But he assumed that the only purpose served by this oxidation was the production of *heat;* affirming that all the *mechanical force* exerted by the Animal body is the product of a transformation of *living* muscular fibre into *dead*, the "vital force" which was possessed by the living tissue being "expended in the shape of motion." The oxidation of the dead material he seems to have regarded as only a consequence of its loss of power to resist chemical agencies, and as a means of its removal,—serving, at the same time, as an additional source of heat. And rightly judging that this chemical metamorphosis should show itself by the increased excretion of urea in the urine (the kidneys furnishing the channel through which most of the nitrogenous "waste" is carried out of the body), he appealed, in support of his doctrine, to what he supposed to be the fact of such increase, and its proportion to the amount of work done.

Notwithstanding the general acceptance of Liebig's doctrine among the Physiologists of that time, there were those who saw that it was attended with considerable difficulties, notably as regards the amount of work done by man and animals upon a very small allowance of nitrogenous food. It was well known, for instance, that Bengalee labourers (who, for their size, are very fair workers*) live all but exclusively on rice, which consists almost entirely of starch; a pound of this, with a little ghee (butter), or a small bit of fish, constituting their ordinary diet. And it appeared from the carefully conducted experiments of Mr. Joule, of Manchester, that the quantity of work done by a grass- or hay-fed ox (taking into account that done by its heart *within* its body, as well as that done *outside* its body) was more than could be accounted-for by the metamorphosis of the nitrogenous constituents of its food.

Mr. Joule was at that time engaged upon an inquiry into the Mechanical equivalent of Heat, which led him to suspect that the combustive oxidation of the non-nitrogenous constituents of food might be a source of *mechanical power*, as well as of heat. But this doctrine was first definitely stated, and shown to be one expression of the great general fact (or law) now known as the Conservation of Energy, in the "epoch-making" treatise published in 1845, by Mayer; who urged (1) that the chemical force contained in the ingested food and in the inhaled oxygen, is the source of the motion and heat which are the two products of animal life; and (2) that the production of these forces varies in amount with the chemical changes to which it is due. The animal body, he maintained, is comparable, in these respects, to a steam-engine, in which work is being done, and heat produced, by the combustion of fuel; and he calculated that the quantity of carbon burnt off by the body in a day (as measured by the amount of carbonic acid given off in the expired air) is far more than sufficient for the whole day's work,—about four-fifths of it being used in the production of heat, while the remaining fifth suffices for the production of the muscular force ordinarily exerted within and without the body. He affirmed that "the muscle is the instrument by which chemical change is transformed into mechanical effect, not the material which is itself transformed;" and he regarded the blood-stream that flows through the capillaries of the muscle, as bringing both the fuel and the oxygen needed to burn it. With the prevision which marks true genius, Mayer asserted that as soon as experimental methods should become sufficiently perfect to render it possible to determine with precision the amount of chemical change, either in the whole animal body or in a single muscle, during a given period, and to measure the production of heat and the work done during the same period, the result would show a definite correlation between them.

This (as will be shown hereafter) has been completely verified by subsequent research; the only point in which Mayer's doctrine has been found to need correction, being one of secondary importance.

*(To be continued.)*

* I have been informed by an engineer who superintended the construction of part of the Bengal system of railways, that the average quantity of earthwork done by a native labourer on the above diet is about *two-thirds* that of a highly-fed English "navvy."

## ARE WOMEN INFERIOR TO MEN?

A FRENCHMAN named Delaunay has recently published a pamphlet, the object of which is to show that women are intellectually and physically inferior to men. At the outset it should be noticed that this M. Delaunay must not be mistaken for the great French astronomer Delaunay, (who died some nine years ago). The point requires noting, because the present Delaunay has ere this dealt with statistical evidence relating to astronomical matters, endeavouring to prove therefrom that the planets Jupiter and Saturn exercise a marked influence on the phenomena of earthquakes. It is hardly necessary, perhaps, to say that the proof was hardly so perfect as M. Delaunay imagined. In fact, his reasoning would not have attracted attention if his name had not sounded like that of a very eminent astronomer, of whose death many had not heard.

M. Delaunay now devotes himself to the task of showing that those who regard the two sexes as intellectually or morally equal, are absurdly sentimental. He seeks to show that women are in a lower stage of development than men, on grounds which he describes as purely anthropological, though he should rather, it would seem, have described them as biological, since his inferences respecting the tests of development are derived quite as much from the study of other animals as of men. He admits that among certain lower forms of life, as insects, some fishes, and reptiles, the females show a superiority to the males; but among the higher races of vertebrates (backboned animals) it is different. Among birds and mammals (he might have added marsupials, or pouched animals, as kangaroos, opossums, &c.), the male is nearly always superior to the female. The inference is, that whereas in lower races the female is superior to the male, the male is equal to the female in races more advanced, and superior to the female in all the superior species. "The supremacy of the female is *therefore*," he says, "the first form of the evolution undergone by sexuality, while the supremacy of the male is the last form." The conclusion is calculated to be so satisfactory to men that they may be permitted, perhaps, to pass over the manifest weakness of the reasoning. The law, which should be universal to be of avail, is admitted to be only general; and it is noteworthy that the reasoning really points to the conclusion that the higher the race, the lower relatively is the female: so that it would seem that the first

XUM

point to be established by one who would prove his nation in advance of others, should be the inferiority of the women of his race. If this were really so, we imagine that there are some races of men, not supposed to be far from the very forefront of human progress, who would not care, at such a cost, to establish their claims to a high position.

M. Delaunay is careful to show why he thinks the female inferior to the male in the higher races. First, we find that among birds and mammals, including man (who shares with the pig, the monkey, the dog, &c., the privilege of breast nourishment), the nutritive phenomena are more intense than in the female. Man eats more than woman; and he is, therefore, incontestably her superior. "Yet, although she eats less, the woman is more of a *gourmande*, and eats more frequently." (This may be proved by comparing aldermanic feasts and club dinners with the favourite forms of feminine gourmandism.) "Next we observe that the breathing of men is more intense than that of women. For an equal height he has a greater capacity of chest, and a larger thoracic index than the woman" (a greater breathing capacity). "He also absorbs more oxygen, though his breathing is slower." (Why should not this be regarded as a proof of inferiority? Suppose a woman were writing, and should put it this way, Woman requires less oxygen than man, and is therefore superior to him; how then?) "The temperature is higher in man than in woman, and the pressure of the blood greater, though the pulse is less frequent." This, again, might be quoted the other way by women. It would be almost as logical to say, Man is superior to woman because he wears a beard, and the hair on his head is shorter than hers.

It appears that the frame of man, as compared with that of woman, proves incontestably his superiority. He has a heavier skeleton, absolutely as well as relatively. "The woman in all the physical characteristics of her skeleton is intermediate between the child and the man, according to Topinaud." Must we then rank the elephant as man's superior? his skeleton is certainly heavier than man's, not only absolutely but relatively. "The woman is not so right-handed as the man; the pre-eminence of the right side over the left is not so marked with her as with the male." Charles Reade would probably regard this as evidence of superiority rather than inferiority; and many men who have not been thought inferior to their fellows, but the reverse, have been at no small pains to train the left side to equality, or as near equality as they could attain, with the right side.

Worse remains, however, to be told. "The male externally is always larger than the female. The woman is not so heavy as the man, although she would often appear larger on account of the development of the adipose system, which in her is greater than the man."

The next point, if established satisfactorily, would be rather more to the purpose. "In all our Hindo-European races," says Topinaud, "the woman is more prognathous than the man," that is, her jaws are relatively more projecting than those of man. This would be a strong point, because there really appears to be a correlation between the position of the jaws in animals and the capacity (relative, of course,) of their brains. But it would be desirable to have all the particulars by which the peculiarity referred to has been established. It would be easy to select classes either of men or of women who would be very unfair representatives of their sex taken as a whole; and though I would be far from saying that anything of this sort has been done, I would note that it is a much more difficult matter than many would suppose (who have not tried) to obtain a large number of either sex who could be regarded as absolutely free from class influences. The task would be easier if men and women pursued similar avocations. As they do not, the statistics collected by Torpinaud may, for aught that appears, have tended, if rightly interpreted, to show that such and such avocations either affected intellectual development, or were selected by persons of inferior intellect, rather than anything specifically distinguishing men from women."

A similar objection applies, but yet more strongly, to M. Delaunay's next point. He says that, "according to his own researches, woman is more flat-footed than man, and has a foot less arched—a sign of inferiority—hence the preference of women for high-heeled boots." It is a rather bold assumption that the form of the feet thus indicates intellectual development. But assuming that it were so, I apprehend that the statistics of feet-forms would show very different results according to the classes that might be selected. Supposing, for instance, M. Delaunay had measured the feet of a few hundred waiters, and compared them with those of a few hundred opera-dancers, he might, perhaps, have inferred either that opera-dancing conduced far more than waiting at table to intellectual development, or else that women were far superior to men. I do not say that a selection so unlikely to lead to a correct result as this has actually been made; but we require to know much more than we do about M. Delaunay's statistics before we can accept his conclusion, even if we admit that the mental and moral qualities can be deduced from the shape of the feet. The fancy for wearing high-heeled boots may reasonably be regarded as showing that women regard a high instep as a natural feminine beauty, to be enhanced where present or imitated where wanting; that, in fact, women are more particular in this respect than men. So viewed, it would no more prove that women are commonly flat-footed than their wearing chignons or switches would show that they commonly have shorter hair than men. In fact, M. Delaunay's argument here is decidedly unfavourable to his theory, if a high instep really is a proof of intellectual superiority; for women try more to enhance those qualities which they regard as feminine, and therefore attractive, than those which they share with men.

"The female voice is sharper than that of the male. Both in wild and domesticated animals the male has the muscular system more developed than the female." All this may be granted; but it proves nothing to the purpose. The bull has a deeper voice than man, and the gorilla has the muscular system far more developed; but we do not infer that the bull or the gorilla stands higher in development on either account than man.

"The movements are more precise in man than in woman." (What movements?) "Among pianists of the two sexes the mechanism reaches a much higher degree of perfection in men." As musicians, whether we consider composition or execution, women certainly are surpassed by men; and I may note among the absurdities of our system of education that almost every girl in the middle and upper classes "learns the piano" (save the mark!) though not one in a hundred has any natural aptitude for music, while among boys music is comparatively neglected. There is little, however, in feminine execution to justify the statement that their movements are less precise. On the contrary, numbers of girls acquire wonderful precision in playing; only, unfortunately, most of them want that aptitude for music which can alone make mere perfection of digital mechanism of any account. It is not easy, however, to see why musical skill should be regarded as a test of mental or moral qualities. I am disposed, on the contrary, to consider that women in our day are handicapped by their musical education; and that pro-

XUM

bably, if so many were not compelled (without musical talent to help them) to waste many years of their life in the weary work of "practising," we might have better opportunities of learning what women are capable of in other ways than we are actually afforded. Music, at any rate, is one of the weak points of modern feminine education, if not its worst feature. Even as musicians, women would be far likelier to show power, if only those who possessed aptitude for music received a musical training. How can the real musician among fifty girls get any chance, when she has to go through, with the forty-nine who are not musicians, the weary music-killing work of our present system of so-called musical training? (Is not this matched, however, by the existing systems of classical and mathematical training for boys, irrespective altogether of any aptitude for classical or mathematical work?)

We come next, in M. Delaunay's paper, to the more important question of cranial capacity and form; for certainly (setting aside, of course, phrenological absurdities), the shape and size of the brain are more likely to indicate mental and moral capacity correctly, than are the shape of the feet, the tone of the voice, and the muscular development. As, however, my limits are already exceeded, I leave the rest of M. Delaunay's paper to another occasion.

So far as we have gone, M. Delaunay's arguments remind us, more than we should have thought possible in a scientific *brochure*, of the reasoning in a humorous article which appeared a year or two ago in the *New York Times*, wherein it was gravely argued that the inferiority of woman to man is proved conclusively by women's habit of sitting on the ground to take off, or to put on, their shoes and stockings. In fact, I am not sure that M. Delaunay might not find more in favour of his theory in this argument than in any of his own that have thus far been considered.

## COMETS.

THE year which was to have seen the end of the world, because of planetary conjunctions and perihelion passages, because Mother Shipton had said so (or was said to have said so), and because the ascending gallery in the Great Pyramid is 1882 inches long (so that the year 1882 is to introduce a new era), has been remarkable in astronomical annals for the number of comets which have been seen. Already six have been numbered, and the year is not over yet. Something still remaining—more, indeed, than we are always ready to admit—of old superstitions respecting comets, has led many to regard the coincidence as full of meaning. Others, not quite so credulous, have supposed that though comets may not come in flights of half-a-dozen together to portend the end of the world, they may yet affect our weather in some way; perhaps directly, as the moon is supposed to do (with very little reason); perhaps indirectly, by acting on the sun. To the astronomer the appearance of so many comets—some of them large ones—has been full of interest, because he hopes by the application of the new methods of research discovered within the last quarter of a century to solve some of the mysteries with which the whole subject is still fraught, despite a number of interesting discoveries which have recently been made.

A brief inquiry into some of the facts which have been discovered respecting comets, and a discussion of some of those peculiarities which still remain among the greatest mysteries of science, will probably prove acceptable at the present time, when comets attract so much interest and attention.

Elsewhere in the solar system we meet with relations not differing greatly in kind from those presented by our own earth. We see a set of globular bodies revolving around the sun in nearly circular orbits, nearly in one plane, and all in the same direction; we find that these globes rotate upon their axis—still in the same direction; they have, apparently, atmospheres proportioned to their dimensions; and many of them are attended upon by bodies resembling our own moon. And therefore, without entering upon the vexed question of the plurality of worlds, we are able to pronounce that, *if* these globes are inhabited, dwellers upon them have, like us, their years, their days, their seasons; a sun—rising in the east and setting in the west; twilight and moonlight; air and vapour; winds and rain; all things, in fact, as it would seem, necessary to their comfort and convenience. Here and there—as in the zone of asteroids and the rings of Saturn—we meet with novelty of arrangement or configuration; but even then we find a stability, either of figure or motion, which renders such objects comparable, so to speak, with those we are accustomed to.

But with comets the case is wholly different. When we have said that these objects obey the law of gravity, we have mentioned the only circumstance—as it would appear—in which they conform to the relations observed in terrestrial and planetary arrangements. And even this law—the widest yet revealed to man—they seem to obey half unwillingly. We see the head of a comet tracing out systematically enough its proper orbit, while the comet's tail is all unruly and disobedient.

The paths followed by comets show no resemblance either to the planetary orbits or to each other. Here we see a comet travelling in a path of moderate extent and not very eccentric; there another which rushes from a distance of two or three thousand millions of miles, approaches the sun with ever-increasing velocity until nearer to him than parts of his own corona (as seen in eclipses), sweeps around him with inconceivable rapidity, and makes off again to where the aphelion of its orbit lies far out in space beyond the most distant known planet, Neptune. Some comets travel in a direct, others in a retrograde, path; a few near the plane of the earth's orbit, many in planes showing every variety of inclination. Some comets regularly return after intervals of a few years; some after hundreds of years; others are only seen once or twice, and then unaccountably vanish; and not a few show by the paths they follow that they have come from interstellar space to pay our system but a single visit, passing out again to traverse we know not what other systems or regions.

The ancients believed comets to be of the same nature as meteors, or shooting stars—either in the earth's atmosphere, not far above the clouds, or, at all events, much lower than the moon. These views are, however, much less ancient than the more correct views maintained by the Pythagoreans. Their doctrine was that comets are planetary objects, having long periods of revolution. From whom this opinion was derived is uncertain. Like other opinions attributed to Pythagoras, it was doubtless obtained from Eastern philosophers; but of what country—whether Egyptian, Persian, Indian, or Chaldæan—we have no means of learning. Apollonius, the Myndian, ascribes the opinion to the Chaldæans. He says they spoke of comets as of travellers penetrating far into the upper (or most distant) celestial spaces. Seneca and Pliny held similar views, exhibiting in this respect, says Humboldt, the imitative faculty of the Romans. But the Greek

philosopher preferred to look for a theory of the universe in the conceptions of his own brilliant and imaginative mind. As if to show future ages how little was likely to be achieved by the highest mental powers without the habit of patient observation, he endeavoured to educe a system of philosophy from fancies, and to found it upon syllogisms. Aristotle—who may be considered the typical philosopher of the Greek school—included comets in the wide range of phenomena which he claimed the privilege of explaining. To him was due the opinion mentioned above—an opinion confidently maintained during the many centuries in which the philosophy of Aristotle held sway over men's minds. To him, also, was due a yet more remarkable opinion, the view, namely, that the Milky Way is a vast comet which continually reproduces itself! Xenophanes and Theon, in the fifth century, adopted a rather singular view of the Aristotelian theory of comets, when they spoke of these objects as "travelling light-clouds."

To these fancies the ancients added the idea that the shapes of comets indicated their character as portents. Thus in Fig. 1 five views of comets are shown, as an arrow-head, a sea monster, a sword, a lance, and in flames.

Fig. 1.—Various fanciful views of Comets, according to Pliny.
From the *Cometographia* of Hevelius.

Tycho Brahe was the first to express doubts respecting the views of Aristotle. From a careful series of observations, he demonstrated that the orbits of comets are certainly situated beyond the moon's orbit. He thought the orbits must be circular, for he lived at a time when none but circular orbits were conceded to the celestial bodies. Dörfel, a native of Upper Saxony, proved that the orbits of comets are either very elongated ovals, or parabolas, and that the sun occupies a focus of the curve. It happens, singularly enough, that this discovery was effected but a year or two before Newton propounded the theory of gravitation. Newton himself examined the orbit of the great comet of 1680 (known as "Newton's comet") and others; and he found that they all accord with the law of gravity.

But before long, Newton's friend and pupil, Halley, effected a yet more remarkable discovery. In hopes of confirming Newton's views by results founded on actual observation, he collected all the records of comets which seemed entitled to confidence, and attempted—as well as his meagre materials would allow him—to calculate the elements of their orbits. In this way he computed the paths of no less than twenty-four. Among these, three presented a remarkable similarity. One appeared in 1531, and was described by Appian; another appeared in 1607, and was observed by Kepler; the third was traced by Halley himself in 1682. The equality of the intervals between these epochs led to the suspicion that the same comet had appeared three times. And Halley found, on searching historical records, that a comet appeared in 1305, another in 1380, and a third in 1456. Combining these appearances with those mentioned before, he thought he had satisfactory evidence of identity. For he was sufficiently familiar with the results which might be expected to flow from the law of gravity, to be aware that *absolute* regularity of motion was not to be expected in a body traversing the solar system in an eccentric orbit, and swayed from its proper path by the attraction of such giant planets as Jupiter and Saturn. Indeed it happens, singularly enough—one out of many remarkable coincidences in the history of comets—that the comet of 1830 was *not* Halley's comet, which really appeared in 1378, a date bringing in a yet greater discordance in the intervals than Halley had suspected and accounted for. With remarkable acumen—since no means existed in his day for anything like accurate computation—he not only pointed out the possible influence of the great planets in disturbing the comet in past revolutions, but he made a rough approach to an estimate of the effect that they would have on the period of its next visit. "Instead of appearing in August, 1757, as it would if its period remained unaltered, it will not appear," he said, "until the end of 1758, or the beginning of 1759, for it will be retarded by the action of Jupiter. Wherefore," he adds, with a pardonable anxiety to secure the credit of his ingenious investigations, "if it should return, according to our prediction, impartial posterity will not refuse to acknowledge that this was discovered by an Englishman."

As the time for the fulfilment of the prediction approached, an intense interest was excited in the minds of astronomers. In 1757, Clairut, Lalande, and Madame Lepaute undertook the computation of the epoch at which the comet might be expected to return. They applied methods of investigation invented by Clairaut himself. It resulted from their laborious computations that April 13, 1759, was fixed on for the epoch at which the comet should attain its closest approach to the sun, or, as it is technically expressed, should pass its perihelion. But Clairaut was careful to allow a month either way, on account of unavoidable omissions in the calculation, and for the effects of unknown forces, "such as the action of some planet too far off to be seen" (a happy anticipation of modern discoveries).

And now the heavens were swept diligently by all the telescopists of Europe, each eager to be the first to

announce the discovery of an object whose appearance or non-appearance was to confirm or to disprove the Newtonian theory. It was actually discovered, however, without telescopic aid, by a Saxon farmer, George Palitsch, on Christmas-day, 1758 It reached its perihelion on March 13, 1759, confirming at once the accuracy of Clairaut's computations, and the justice of his caution in assigning rather wide limits of error.

It was now evident that comets travel, like the planets, in determined paths; and also, that the investigation of their motions is a subject worthy the study of the ablest mathematicians, and sufficient to tax their highest powers. An account of their labours would be out of place in such an article as the present; but we recommend the subject to the notice of the agricultural student, as one of the most interesting chapters in the history of modern science.

One comet, however, discovered not long after astronomy had achieved this triumph, seemed at first to teach a different lesson. In 1770 a comet appeared whose path turned out to be—not a long oval or parabola, as had been the case with all the orbits yet examined—but an ellipse of moderate extent, and not very eccentric. The orbit lay also much closer than usual to that thin slice of space (so to speak) within which the planets are observed to move. Lexell, who computed the path, found that the period of the comet was about five-and-a-half years. Its return was carefully watched for, *but no one has ever seen the comet since.* The cause of its disappearance, and also of its sudden appearance—for this was equally remarkable, when we remember that so conspicuous a comet could not have been circulating long in its small orbit without discovery—was carefully inquired into. The result was singular. On tracing back the path of the comet, it was found that it must have passed very near to the great planet Jupiter. "It had intruded," says Herschel, "an uninvited guest into his family circle—actually nearer to him than his fourth satellite." Accordingly, the comet's path, originally a long oval, had been bent into a curve of less extent. Having once entered on this new path, the comet was free to follow it—always returning, be it noticed, to the point at which it had started on it—so long as Jupiter was not interfered with. But it happened, unfortunately for the stability of the comet's motions, that, after going twice round the new path, it again presented itself near Jupiter's track, when the planet (which had meanwhile gone once round his orbit) was not very far from the scene of his former encounter. He accordingly again exerted his influence upon the unfortunate comet, and this time dismissed it on a path which will not admit of its approaching the earth near enough to be seen.*

Let us return, however, to Halley's comet.

It so chances that the comet which was the first to show full obedience to the law of gravitation, was one which exhibited in a very remarkable and significant manner the characteristics which distinguish comets from other heavenly bodies, and make them so mysterious to the student of science. At the return of Halley's comet, in 1836, all that had signalised the return in 1759 was repeated, but the mathematical triumph was far greater. Damoiseau, Rosenberger, and Pontecoulant calculated the comet's return to perihelion within two or three days, instead of a month, and the time when it passed this point of its orbit corresponded, within a few hours, to the mean of their several estimates. On the northen heavens where it was first seen, the comet presented a remarkable appearance, with a long and brilliant tail stretching over an arc of many degrees upon the sky. When it had passed from our northern skies, it was carried (after a short interval, during which it was lost to view in the sun's rays) to the southern heavens. Sir John Herschel, and Maclear (Astronomer Royal at the Cape), were prepared to receive it; but when first observed by them it showed none of the features which made it so remarkable in our skies. It had no tail and scarely any head. In fact, Sir John Herschel, in one account, says, that as first seen it could only be distinguished from a fixed star by its motion. The study of its gradual change of aspect from that time threw so much light on the nature of comets' tails and other appendages (or at any rate of that particular comet's tail) that Sir John Herschel, not accustomed to be over confident, said there could be no doubt as to the true interpretation of the observed phenomena. What these phenomena were shall be considered further on.

* It must be noticed, however, that Leverrier, who very carefully re-examined the question, was led to question the accuracy of the results recorded above. Admitting that Jupiter had twice disturbed the comet, he thinks there is no certainty (for want of sufficiently accurate observations) respecting either the original path of the comet, or that in which it is at present circulating unobserved—if, indeed, it has not been *absorbed* by Jupiter.

## ILLUSIONS.

By Thomas Foster.

THE senses are the means by which, directly or indirectly, all observations are made, and science can only make real advance in so far as it is based on observation and experiment. It is most important, therefore, that either our senses should be trustworthy in their action, that is should give us true information, or (if they neither are absolutely trustworthy originally nor can be so trained as to become so) that we should be able to test and to correct their indications.

Now it very soon appears, when we put the matter to the test, that the direct evidence of the senses is not to be accepted without careful cross-examination. The science of our day may be regarded as having been established in opposition to the apparently obvious evidence of the senses. Take, for instance, astronomy. Nearly everything that the eyes tell us about the heavenly bodies, and nearly all that the sight and touch tell us about the earth (so far as astronomy has to deal with the earth as one of the planets) is false. Not one of all the stars we see in the skies is really where we see it. The earth seems flat, large, and fixed; it is really a globe, small compared with the seemingly small stars, and it is moving in many ways, not one of which the senses correctly appreciate. It is the same with other sciences.

We are not concerned, however, to discuss here how far the apparent teaching of the senses has to be analysed before its real meaning can be understood. The examples illustrating this would cover the whole range of science. For instance, to show how the real place of a star can be determined—more or less exactly—from its apparent place in the sky, we require to discuss the laws of refraction, aberration, the proper motion of stars, and a number of other matters. In such cases as this, though what the eye tells us is in a sense incorrect, the eye is supposed to do its work correctly. The eye tells us *truly* that the rays received from the star by it have come in such and such a direction, and what science has really to do is to determine in what direction those rays must have set out in order after various changes of direction, due to the various media through which they passed, to reach the eye situate on a moving and rotating body like the earth, in the direction which they had, or at least seemed to have—or, more

strictly, in order to produce an image of the star on such and such precise part of the retina.

But there are many cases in which the senses seem actually to convey false information, the eye telling us wrongly about the shape, size, position, &c., of an object, the touch deceiving us as to its form and qualities, the hearing, the smell, the taste, each in its own way deceiving us. It is such cases as these that I propose to examine. It is most important for the student of science that he should be aware of the various forms of error into which the direct action of the senses may lead him. I could cite instances where, for want of precisely such information, the student of science has been at the pains to explain a phenomenon which had no real existence, or which was precisely the reverse of reality. Mr. Proctor mentions somewhere a case where a French astronomer (Chacornac), was careful to explain *why* the edge of Jupiter's disc is brighter than the middle, the fact being that the middle is brighter than the edge, the apparent brightness of the edge being a mere delusion. It may be added that the discussion of the class of illusions referred to is full of interest. The reader will find that the careful consideration of the cause of illusions will generally suggest other illusory experiments, sometimes more striking than those here described. There are few occupations more interesting as pastimes, and at the same time more instructive, than the invention and testing of various forms of illusion.

It should be mentioned at the outset that such illusions as these are wrongly called sensory illusions. As Huxley points out, "there is no such thing as a fictitious or delusive sensation. . . . . But the judgments we form respecting the causes and conditions of the sensations of which we are aware, are very often erroneous and delusive enough; and such judgments may be brought about in the domain of every sense, either by artificially-contrived combinations of sensations, or by the influence of unusual conditions of the body itself." He adds, "mankind would be subject to fewer delusions than they are if they constantly bore in mind their liability to false judgment. Men say, 'I felt,' 'I heard,' 'I saw' such and such a thing, when, in ninety-nine cases out of a hundred, what they really mean is, that they judge that certain sensations of touch, hearing, or sight, of which they were conscious, were caused by such and such things." It is precisely this lesson which I want to enforce in the present series of papers.

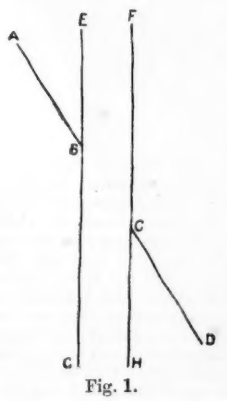

Fig. 1.

The simplest illusions are those affecting straight lines, causing them to appear crooked, or curved, or *vice versa*.

Take, for instance, the case illustrated in Fig. 1. Here the parts of the broken line AD appear to be parallel, but not—as they really are—in the same straight line. The part CD seems lower than the true prolongation of the part AB. The explanation of this is not so obvious as in some less simple cases which I shall consider further on. Manifestly, the mind recognises that, owing to the interposition of the space EH, the point where the prolongation of AB should appear must be lower down than B, but it underestimates the allowance to be made on this account. This is equivalent to saying that the eye under estimates the breadth of the strip EH. Why this should be? It seems to me *it is because the space EH appears to lie* above the plane containing AB and CD, so that the real *breadth of EH is unconsciously* judged to be less than it really is; for, of course, a strip nearer the eye than EH is would *seem* broader than EH if really of the same breadth, and if seeming *no broader* than EH, would *really* be of less breadth. The eye judges that EH *is* nearer, and infers unconsciously that *it is of less breadth than* EH really is.

*Let us test this explanation, first seeing* whether a strip which breaks a curve, as EH breaks the straight line AD, *really seems* nearer to the eye than the curve.

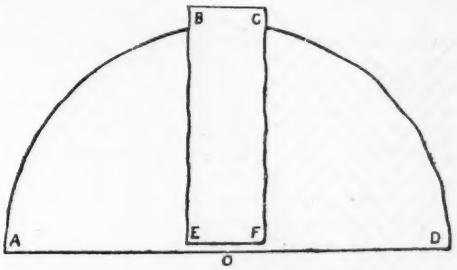

Fig. 2.

Describe a semicircle AD (Fig. 2) broken at BC, and between B and C draw the outline of strip BF as in the figure. We see at once that the space BF appears to be nearer the eye than the portions ABE, CFD. Doubtless the judgment unconsciously reasons that the strip which breaks the outline ABCD must lie upon the semicircle and hide the portions wanting.

Again, it seems to me and to some others, but not to all to whom I have shown Fig. 2, that the figure formed by carrying on the curves AB, DC, to meet over space BC would not be a complete circle, but somewhat contracted horizontally. This would correspond with the above explanation of the illusion illustrated in Fig. 1. As, however, all eyes do not recognise this second illusion of Fig. 2, let us modify the experiment, noting that the same eyes which might not recognise an illusion apparently affecting breadth only, would readily be affected by an illusion affecting direction. (We may remember what Brewster points out in dealing with one of the illusions mentioned in his Natural Magic, that in many cases it is the most observant eye that is affected by an illusion. In the present case the eye which has the best power of estimating breadths would be the one most affected by illusion as to the breadth of the space BF.)

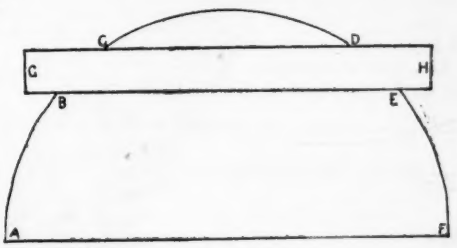

Fig. 3.

Draw then the strip GH, Fig. 3, crossing the semicircle ABDF as shown. Here the curve CD seems to belong to a smaller circle than that of which it really forms part. It looks as though the curves AB, FE caried on beyond the strip GH would lie about considerably outside CD.

It will be found that if the parts CD, BE of the sides of

the strip GH are erased, the illusion ceases. The erasure restores, as it were, the space GH to the level of the paper to which it really belongs.

The case illustrated in Fig. 1 is commonly regarded (but without sufficient reason, or, indeed, any assigned reason) as belonging to the class of illusions illustrated in Fig. 4 (first noticed by Zöllner). Here the lines AB, CD, EF, GH, &c., which are really parallel, appear to converge alternately towards AC, DF, EG, HL, and KM. This illusion is different from that of Fig 1, as affecting the apparent directions of lines, whilst the other does not (parallel lines are regarded by geometricians as in the same direction).

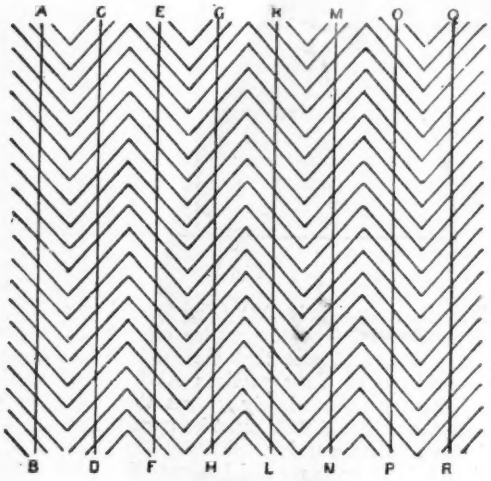


Fig. 4.

The illusion of Fig. 4 appears to result from the attention being drawn unduly to the circumstance that the vertical lines pass athwart the other sets at an angle, so that the angle is, as it were, exaggerated. The eye notices, for instance, that AB passes the parallels in order from right to left, the uppermost parallel crossed by AB being farther to the right than the lowest, and thus the idea is conveyed that B, instead of being a point vertically below A, is to the left of such a point. So D seems to the right of a point vertically below C, and so on.

The illusion of Fig. 4 admits of being varied and, in some cases, strengthened by substituting waving lines of various forms for the diagonal hatching. Several cases of this kind will be shown in the next number. I conclude this paper with a few simple illusions, affecting not only the direction and position of lines really straight, but also their straightness, making them appear as curved lines.

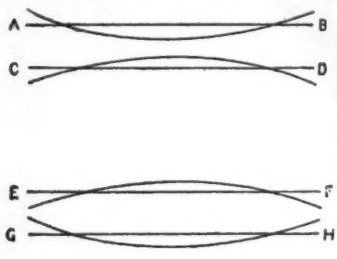


Fig. 5.

The lines AB, CD of Fig. 5 appear to be curved so as to be nearer at the middle than at either end; while the lines EF, GH, in the same figure, appear to be so curved as to be farther apart in the middle than at either end. Obviously, the eye unconsciously notes the varying distance between the really curved lines and the straight lines, and attributes part of the change of distance to a curvature of the really straight lines.

This form of illusion may be modified, as shown in Fig. 6. Some find the apparent curvature produced by the arrangement of Fig. 6 stronger than the other. For my own part, I find the apparent curvature of the lines AB, CD strongest in Fig. 5, that of the lines EF, GH strongest in Fig. 6.

The illusion seems intensified in the case of lines AB (Fig. 7), which seem to be convex towards each other; the symmetry of the pair of curves lying between these parallels ought theoretically to restore the idea of parallelism. The lines EF and GH in the same figure may be made to seem either parallel or concave toward each other, according as the eye takes together the curves which approach EF and GH (respectively) nearest, or the concentric pairs; for the two curves which lie between EF and GH produce opposite effects on each of the lines EF, GH.

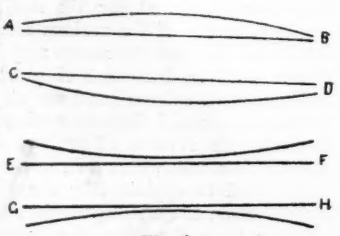


Fig. 6.

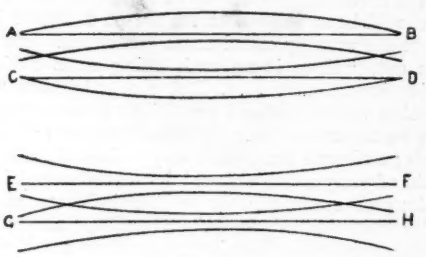


Fig. 7.

Next week I shall consider other cases of apparent curvature, but in the meantime I may note that I shall be glad to hear from readers who have noticed illusions akin to these, or may have been led to other explanations than those I have suggested.

---

WHAT is a man,
If his chief good, and market of his time,
Be but to sleep and feed? A beast, no more.
Sure, He that made us with such large discourse,
Looking before and after, gave us not
That capability and godlike reason
To fust in us unused.—*Shakespeare.*

ELECTRICAL EXHIBITIONS AND THE GOVERNMENT.—Our correspondent probably reflects the general opinion of British exhibitors in Paris when he says that "the conduct of our Government has preventcd our country from holding the place it deserved to hold among European nations in an industry which promises before long to be one of the utmost importance." But it is perfectly well known to foreigners, and also to ourselves, that the British exhibitor, so to speak, must swim without corks. He may not make so large a show as some of his rivals; but what he does show is even more likely to be over-estimated, as being the result of his unaided efforts, than to be undervalued in consequence of any unfair comparison with those who have enjoyed advantages which he does not possess. The difference between the conditions which surround British and foreign exhibitors is thoroughly understood by all who are interested in understanding it; and we do not believe that a steady adherence to the general rule of Government non-intervention, although it may detract from the prettiness or completeness of a display, will ever be really detrimental to the interests of a national manufacture.—*Times.*

# THE EASTERN SKIES IN NOVEMBER.

*This Map shows the position of the stars in the Eastern Skies:* —

**On October 30, at 11½ o'clock. | On November 7, at 11 o'clock. | On November 14, at 10½ o'clock. | On November 22, at 10 o'clock. | On November 29, at 9½ o'clock.**
**On November 3, at 11¼ o'clock. | On November 10, at 10¾ o'clock. | On November 18, at 10¼ o'clock. | On November 25, at 9¾ o'clock. | On December 3, at 9¼ o'clock.**

The movements of the planets Mars, Jupiter, Saturn, and Neptune, which are travelling in this part of the stellar heavens at present, are indicated by the lines marked respectively with the names of these planets. The planet Mars, it will be noticed, passes, during this month, what is called a stationary point; he is advancing (moving from right to left) till Nov. 17, after which he retrogrades. But he does not really come to apparent rest, owing to the wide spread of the loop he forms between his stationary points. Jupiter, Saturn, and Neptune are all three retrograding. They are in opposition, or exactly opposite the sun, so that they come to the south at midnight on the following days:—Saturn on Nov. 1, Neptune on Nov. 7, Jupiter on Nov. 13.

## DARWIN ON MOULD AND WORMS.*

NO man of science of our day understands better, or applies more thoroughly, than Darwin, the principle laid down by Lord Bacon, that "Man, as the minister and interpreter of nature, does and understands as much as his observations on the order of nature permit him, and neither knows nor is capable of more." To one who rightly apprehends this, the fundamental principle of modern scientific research, small things and great, so only that they illustrate the order of nature, are alike worthy of study. He may carry his survey over the depths of space, or into the structure of a microscopic creature; he may extend his view into the remote past and the distant future, or he may limit the range of his vision to phenomena taking place in a second or in a yet shorter time: but only in so far as his purpose is to determine the order of nature's works, is he the true minister and interpreter of nature. The modern student of science, following this principle, is in strong contrast with the philosophers of the Greek school, who, little disposed to pursue observations, evinced, as Humboldt has said, "inexhaustible fertility in giving the most varied interpretation of half-perceived facts;" and, as Bacon himself said, "Laid their whole stress upon intense meditation, and a continual exercise and perpetual agitation of the mind;" and so were led to frame systems on insufficent knowledge, and to explain false systems by false hypotheses. Doubtless, a philosopher of that school would have looked with contempt on a Darwin studying the movements of plants, the ways of bees, the breeds of pigeons, and analysing the play of features in joy or in sorrow, in anger or in pleasure. It would have seemed to him far worthier to educe from his moral consciousness ideas as to the true position of worms in the scale of being, than to devote years to the actual study of their ways and works. But by the humbler and more laborious method the student of science in our day manages to attain, or at least approach, the truth; the more brilliant philosopher of the olden school educed from his active mind multitudinous errors.

Darwin's latest treatise, on Vegetable Mould and Earthworms, affords perhaps the best illustration of his method of all the works that he has yet published. His "Origin of Species" and "Descent of Man" mark an epoch in science; but such a work as the present illustrates the way by which the new paths have been entered. It is true no one can read those epoch-making works without recognising in every page the kind of work on which their author's mind has been engaged while establishing his theories, or the tone of modern scientific thought. But results have there to be touched on which, in a work like his "Monograph on the Cirripedia," "The Movements and Habits of Climbing Plants," and, above all, the present treatise, are exhibited in detail.

*Forty-four* years ago Darwin announced the first results of his study of the formation of vegetable mould, in a paper read before the Geological Society of London. In the interval which has elapsed since then he has been accumulating the stores of knowledge about mould and mould-makers which are presented in the work now before us.

Perhaps the most remarkable results of Darwin's observations is the "stupendous work" accomplished by creatures so small and weak. It was objected against the views which he published in 1837, that worms could not possibly bury to a depth of several inches fragments of cinders, burnt marl, &c., which had been strewn over the surface of meadow land. But now Darwin is able to speak confidently of their burial of the remains of Roman villas and pavements. He shows also how ancient encampments and tumuli have been gradually lowered by the agency of worms. Grass-covered slopes undergo perpetual denudation through their operations, the covering of grass remaining all the time intact, and even the inclination of the slope remaining unchanged. It may well seem incredible to the superficial reasoner, that creatures like worms,—small, weak, and soft-bodied—should produce such results; nay, results far greater in the course of time, changing as they do the entire aspect of a country. It is this inability, as Darwin well remarks, "to sum up the effects of a continually-recurring cause, which has often retarded the progress of science, as formerly in the case of geology, and more recently in that of the principle of evolution." When men like Sir John Herschel or Sir Charles Lyell have spoken of the effects of slowly-acting causes in modifying continents and seas, they have been ridiculed by the thoughtless, who cannot see how the downfall of rain, the slow movement of rivers, the play of waves on shore-lines, can produce such results. In like manner the biologist is ridiculed who, noting small changes in various races in short periods (or even in periods which to our conceptions seem long), points to the effect of such changes when multiplied during the lapse of those long periods of time of which the earth's crust tells us. But our author has shown how even creatures so tiny and weak as the coral animal have made large islands and long lines of sea-resisting reef, by constant labour; and now he shows how under our very feet the despised earthworm is changing the form and nature of the land we live on. When we learn that the rich dark mould in which vegetation thrives best, is made by worms, we see that not only the aspect of a country, but the condition of its inhabitants, and even its history, have been modified by their work. So that we may accept in its widest significance his remark that "it may be doubted whether there are many other animals which have played so important a part in the history of the world as have these lowly creatures."

The study of the habits of worms in this work is full of interest. As in nearly all the author's books, the language is clear and simple. It may be said indeed of this treatise, presenting the fruit of observations so long continued on a subject apparently so little promising, that great though its scientific value unquestionably is, it is better fitted than nine-tenths of our works of fiction to while away a weary hour. It merits, however, more than mere reading. It is a work not to be tasted merely, but to be chewed and digested.

We shall hereafter return to this work, to consider more at length some of the interesting results of Mr. Darwin's researches.

* "The Formation of Vegetable Mould through the Action of Worms, with Observations on their Habits." By CHARLES DARWIN, LL.D., F.R.S. (London: John Murray, 1881.)

---

APPLICATIONS OF ELECTRICITY.—The public hardly realise, as yet, a tenth part of the uses to which electricity can be readily and conveniently adapted; and exhibitions will furnish the best possible means of rendering them familiar with these uses, which, in many cases, are of an essentially domestic character. Electric bells, for example, although almost universal in large hotels, offices, and public buildings, make way but slowly in private houses, notwithstanding their many advantages. Telephones, in like manner, are far less numerous than they ought to be; and many forms of electrical arrangement quite common in the United States, and found to have great value in saving labour, are scarcely at all in use among ourselves. The electric light is not yet employed in many places for which it is eminently suitable; and its employment is still impeded by difficulties of detail which a larger demand would set aside.—*Times*.

## Letters to the Editor.

*[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]*

*All Editorial communications should be addressed to the Editor of* KNOWLEDGE; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

*All Remittances, Cheques, and Post-Office Orders should be made payable to Messrs. Wyman & Sons.*

**** All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.*

*All Letters or Queries to the Editor which require attention in the current issue of* KNOWLEDGE, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

## Our Correspondence Columns.

I AM very anxious that Correspondence should become a distinguishing feature of this magazine. I wish all readers to feel that in these columns, including the section for Queries and Replies, they have a means of resolving doubts which may occur to them in scientific study or investigation, when reading articles on science in magazines and journals, and in studying the pages of this magazine itself. Our space will indeed be too limited to permit of our dealing with all such questions as occur to students; so that simple and easily-resolved questions cannot occupy space in these pages, which could be better employed. Those who ask such questions must not be angry if they find a reply in our "Letter Box" couched in very brief terms. Still the wish of myself and others, who will join with me in conducting these columns, will be to leave, if possible, no question unanswered. And as we shall in many cases leave reply to readers who may have special means of information on particular subjects, so also shall we often join the ranks of those who ask questions.

A valued correspondent, who will, I hope and believe, help largely in dealing with difficulties which come into these columns for discussion, advises (see letter 1) that paradoxers should be rigidly excluded at the outset. He has had a very wide experience in this matter; but mine has been even wider, and I must confess to still feeling some tenderness for paradoxers. So many of them have originally been victims of ill-written text-books, difficulties left unexplained, and so forth, that hopeless though the attempt may seem of putting them on the right track, I do not yet feel disposed to give it up entirely in every case. In these pages the honest paradoxer, at any rate, may at least state his difficulties; but space will not be given to him to urge theories in defiance of known facts or established doctrines. I shall venture to ask that even those who are surest of their ground in meeting paradoxers will deal tenderly with these weaker brethren. The paradoxer finds it hard enough to give up a theory which he has, perhaps, nursed for years in the belief that it was legitimate, without being loudly ridiculed or harshly rebuked. (*Nescit vox missa reverti*; were it otherwise there are few words of my utterance I would more wish to recall than those in which I have exposed, with unnecessary energy, mistakes which might equally well have been corrected in a gentler manner.)

In more equal arguments, where, perhaps, each party to the discussion has some truth on his side, a greater liveliness of tone may, perhaps, be permissible. Yet, after all, it is nearly always seen that the louder-voiced in a controversy is the one who is in error.

One form of writing, and one alone, we purpose rigidly to exclude from these columns. No personalities will be permitted, whether in the form of attack, of adulation, or of self-seeking.

### HINTS TO CORRESPONDENTS.

[1]—Believing as I do that the scheme laid down in your prospectus is a sound one, and that, if it be carried out in its integrity, KNOWLEDGE cannot fail to have a great future before it, I would, with your permission, crave a little space to put on record certain ideas of mine in connection with one department of your journal—I mean the "Correspondence Column." I am moved to do this from the consideration that the perusal of the similar portion of such of your contemporaries and predecessors as have had anything like a kindred aim with that which you profess, has led me to the conclusion that of all parts of a scientific paper this is the most apt to degenerate into the weakest, unless a tight hand is kept upon those whose chief glory it is to see themselves in print. Amid many earnest students and seekers after knowledge, whose legitimate thirst for information it should be at once the duty and the privilege of the man of science to gratify, are always to be found men who, under pretence of seeking instruction, will obtrude their own "fads" on the public, and unless restrained in the outset, speedily develop into paradoxers of the most aggravated type. Or again, there are the people who put solemnly on records things which are as "familiar in their (and other peoples') mouths as household words," and who will tell you that two and two make four, as though it were a direct and immediate revelation from Heaven. Furthermore, we have the gentlemen who conceive that anything in the shape of the observation of a phenomenon, no matter in how slipshod a style it is described, must necessarily be of the highest scientific interest. These are the people who write to the papers and say that "while crossing Salisbury plain on Friday night between nine and eleven p.m., I noted a sudden illumination of the sky, which, I have no doubt, proceeded from a falling meteorite; inasmuch as, on turning round, I observed a bright object as big as a good-sized stone just disappearing on the horizon." Or "happening to look at the sun on Monday I could detect two spots on it." I would appeal to any who has studied the quasi-scientific correspondence which appears from time to time in different journals, whether I am caricaturing no inconsiderable part of the letters which, in some occult way, their editors suffer to pass? The people, too, who take counsel on matters of almost purely personal interest, who "have built a greenhouse 8 ft. by 4, and will feel obliged if any of your readers will tell me how to keep it stocked with plants throughout the winter." The youths entering into competitive examinations, who wish to be told how to simplify the fraction $a+x+3-\frac{a}{x}$ and so forth. All these occupy space which should be devoted either to those who have something really to learn or something to teach. *The* person, though, to be rigidly excluded and forcibly ejected from the pages of every scientific periodical whatsoever is the paradoxer; the man who has squared the circle with a two-foot rule and a bit of string; who has been down into the Essex marshes with a draining-level, and shown, conclusively, that the earth is as flat as a pancake; or he who, by dividing the number of days in the year by the height of the Lord Hill Monument at Shrewsbury, finds the number 3·1416, and forthwith proclaims that Mr. Haycock, the architect must have been "inspired."* There is a record, more or less authentic, of a man, that his servant ran up to him in great trepidation, exclaiming, "Oh, sir, there is a bailiff down stairs!" "All right," was the response of the master of the house, "ask him to take a chair." "He has taken six already, sir," replied the servant. Upon a cognate principle I would, *in limine*, try to deprecate the offer of a chair in these pages to any paradoxer whatsoever. Depend upon it, should such be made, it will be found that he has taken his "six already."

I am, Sir, yours, &c.,
A FELLOW OF THE ROYAL ASTRONOMICAL SOCIETY.

### IS THE SUN HOT?

[2]—In your lecture on the Sun we are told that the sun is himself hot, and the source of heat for this earth, just as a fire is the source of heat for a room. Now, I would ask how this can possibly be the case, and I would take the very illustration you employ to show how entirely erroneous is the idea that the sun can be the source of the earth's heat, in the sense, at least, in which you speak. If I

* For an amusing travestie of such reasoning, see "The *Tribune* Riddle" further on.—*Ed.* KNOWLEDGE.

XUM

draw nearer to a fire in my room, I get more of its heat—I find myself warmer. But we know perfectly well that by drawing nearer to the sun we get colder instead of warmer. The tops of lofty mountains are nearer to him than the valleys and the plains which lie around their base. Yet, while it is warm in the valleys and the plains, it is intensely cold at the tops of the mountains—so cold that if the mountains are high enough (*and are so much nearer the sun*) the snows never melt there. How can this be if the sun warms the earth as a fire warms a room? Again, if we go up in a balloon, we find that the greater the height to which we ascend the colder is the air.

Indeed, if meteorologists are right in saying, as they do, that the highest clouds, the feathery cirrus clouds, are composed of particles of ice, it becomes simply absurd to maintain that the sun is himself an intensely hot body, for those clouds can be seen on summer days in the full blaze of a solar heat (if that view of his nature were correct). I do not myself understand how meteorologists can have sufficient reason for maintaining that cirrus clouds are composed of ice crystals, unless they believe with me, that the sun is not himself hot (though, of course, he is the source of heat to the earth, acting, I believe, upon the atmosphere in such a way as to generate much heat where the air is dense, and very little where the air is rare); in fact, when I hear the statement made that the cirrus clouds are composed of particles of ice, I recognise another illustration of what I regard as the undue confidence of scientists. They tell us in the same page that cirrus clouds are formed of snow crystals, and (see the accounts of Glaisher's balloon ascents) that even when a balloon is at its highest, the cirrus clouds are still far above, looking no nearer than they appear as seen from the earth's surface. How, then, can the idea that the cirrus clouds are composed of ice crystals be other than a theory, and a very wild theory in my judgment, to be entertained by the very same men of science who believe that the sun is a fire warming the earth?

Although I do not profess to be an astronomer or a meteorologist, I have for many years given great attention to the subject on which I now write; and I have collected together a number of considerations which all tend to show that the solar rays only generate heat when they act in combination with the atmosphere. I would invite readers of KNOWLEDGE to study this question apart from any preconceived ideas they may have, and uninfluenced by the names of so-called authorities in science.

In one respect the theory which I have given above, besides being obviously in better accordance with observed facts than the accepted one (which is, indeed, entirely opposed to them), is much more acceptable to those who recognise the mind of a Supreme Being of infinite wisdom in all the workings of creation. For, observe, if the accepted theory is true, by far the greater part of the sun's rays are wasted. I think Tyndall has stated that more than two thousand millions of rays pass away from the earth for each ray which falls upon it, and that even taking all the planets together, only one ray falls on some planet for two hundred and thirty millions which pass into space and are utterly wasted. Now, what low, and, therefore, what utterly incorrect ideas are given of the great Creator's plans, by a theory which thus tells us that only an exceedingly minute fraction of the work done by the vast orb which He has set to rule and illuminate our solar system is of any use to that system? What should we think of a man who wasted £999 out of an income of £1,000? Yet that would be but a small waste compared with that which scientists assure us is going on in the case of our own sun; and, by parity of reasoning, this waste is repeated millions of times among the millions of solar systems of which astronomers tell us. For my own part, I cannot believe that the picture thus given of the Creator's ways, in the case of these the noblest of the orbs He has made, can be a true one.—I am, sir, yours faithfully, ANTI-GUEBRE.

["Anti-Guebre's" tone is rather dogmatic for one who desires to oppose what he regards as scientific dogmatism. His questions shall not remain unanswered; but we prefer to wait until either he or others who consider the theory of the sun's heat untenable shall have given more of their reasons for want of faith.—ED.]

## CAN ICE-YACHTS SAIL FASTER THAN THE WIND?

[3]—I have seen it stated that the American ice-yachts often travel faster than the wind, and I have been told that in the *Newcastle Weekly Chronicle* you have explained that, though before the wind one of these yachts cannot go faster than the wind, which is, indeed, obvious, yet with a beam wind they will go faster than the wind—sometimes twice as fast. Surely there must be some mistake here. Everyone who has considered the usual explanation of the way in which a ship sails with a side wind knows that the driving force exerted by such a wind is but a part—often but a small part—of the driving force which the same wind would exert on a ship sailing directly before it. In determining the effective force of such a wind we first resolve the full force into two—one perpendicular to the sail, the other parallel to the sail; each of these is less than the full force, being represented by the two sides of a right-angled triangle, while the full force is represented by the hypothenuse. The latter of these, the force parallel to the sails, produces no effect. The force perpendicular to the sail is then resolved into two, one perpendicular to the ship's course, which produces only leeway, the other parallel to that course, which is the only part of the wind's force effective in propelling the ship. Each of the two last-named parts is less than the force perpendicular to the sails, which is itself less than the force of the wind. *A fortiori*, the effective propulsive force of a side wind is less than the full force, and must, therefore, produce a smaller velocity; in other words, the ship sails faster before any given wind than on any other course. What is true of a ship sailing at sea must be equally true of a ship sailing on ice. It seems to me, therefore, sheer absurdity to assert that an ice-boat can go faster than the wind, when we know that, sailing before the wind, she can never have a velocity quite equal to that of the wind.

If there is any flaw in this reasoning I should be glad to have it pointed out. UPSILON.

["Upsilon's" difficulty is a very natural one, and his reasoning seems at a first view just. It is, however, incorrect. It is quite true that regarding a ship as at rest, a wind of given velocity cannot exert a more effective influence than that which it produces as a stern wind. But the same is not true when the ship is in motion. If "Upsilon" compares the two cases—an ice-yacht running before the wind at the same rate as the wind, and one running at the same rate with the same wind abeam—he will see that whereas in the former case the wind exerts no driving action at all on the ice-yacht, there still remains in the latter case a driving force; so that unless the frictional resistance balances this force, the velocity of the yacht will increase. I leave this as an exercise for "Upsilon," but if he should find any difficulty with the problem I will give a fuller explanation, with an illustration or two later.—ED.]

## THE BIRMINGHAM AND MIDLAND INSTITUTE.

SOME EGOTISTICAL REMINISCENCES, BY W. MATTIEU WILLIAMS.

AN intellectual festival was held in Birmingham on the 20th of last month, when a breakfast party, including the Mayor, his brother, the President of the Board of Trade, Dr. Siemens, and other notables, filled the Town Hall, the guests afterwards making procession to inspect and inaugurate the new extension of the Midland Institute; and in the evening another meeting was held in the Town Hall to distribute prizes to the students, and listen to the thoughtful address of Mr. Siemens on the scientific training of artisans.

This Institute having been established for the purpose of doing for the Midland metropolis nearly the same work as KNOWLEDGE promises to do for all English-speaking peoples, a short sketch of the working progress of the institution by its first teacher cannot be out of place in the first number of this magazine.

In 1854 Birmingham was in a condition of intellectual destitution that to a young native of the present day must be difficult to conceive. A Mechanics' Institution had been tried, and had failed. Various other minor efforts of the same kind had met with the same fate, and the old Philosophical Society that at one time had done honourable work, and was, if I remember rightly, associated in its beginnings with the great Priestley and his friends, was dying of inanition, little remaining but its physical home, at No. 7, Cannon-street. It seemed as though the infamous outrages upon Priestley by the "Church and King" mob had left a blighting curse upon the intellect of the town.

But in spite of this there were a few men of strong faith; there was Arthur Ryland, the now justly recognised father of the Institute; there were Captain Tindal, Sir F. E. Scott, Follet Osler, John Jaffray, William Matthews, and others, who refused to despair, and they united to do something worthy of so important a centre as Birmingham. They held meetings, subscribed money, and induced others to do the like. Charles Dickens gave readings in the Town Hall, which left a clear profit of £339. 16s. A comprehensive scheme was drawn out, including a "General Department," to supply the usual elements of a Literary and Scientific Institution for the well-to-do classes, and a "School of Industrial Science," since named "The Industrial Department," in which should be supplied systematic instruction, including "Chemistry," as applied to the various Manufactures and Agriculture, Mechanics, Metallurgy,

Mineralogy, and Geology, Ventilation of Mines, and Mining Engineering." Besides these, there were to be "Museums common to both Departments," and "arrangements for associating the School of Design with the new Institute."

The first beginning of the practical work of the Institute was the formation of three classes, "Class A, Physics; Class B, Chemistry; and Class C, Physiology." They were conducted by myself in the building of the old Philosophical Society in Cannon-street, where I had already delivered an introductory lecture, and commenced work in October, 1854. Six months later I added to these two junior classes (day and evening) for Elementary Physics, and two "Female Classes" for the same subject applied especially to household matters, and other extensions gradually followed.

The public meetings and general agitation of the subject throughout the town brought many students, who began with the enthusiasm of novelty, and held on for awhile, especially in the Chemistry Class, where the first twenty lectures devoted to the metalloids, and including the brilliant combustion experiments, seldom failed to comply with Brande's formula for a successful chemical lecture, "a flash, a bang, and a stink." When, however, we came to the common metals and their prosaic salts, there was a sad falling-off, in spite of the local importance of the subject.

This alarmed the Council, but being behind the scenes myself, I understood it. Presently, however, some truly alarming symptoms appeared. I found that we were exhausting our material, that the whole population of Birmingham only contained a very limited number of artisans and others who could appreciate the advantages and the pleasure of systematic study of science; that we had already nearly satisfied their limited demand, and that the rate of growth of a new generation of students was ruinously slow. The further I extended my inquiries and the greater the efforts made by the best men in our Council to recruit the classes, the more clearly and seriously was the difficulty presented.

What was to be done? Must we follow the too common example of substituting clap-trap for sound instruction in order to maintain an appearance of success, or should we stand firmly by our original intent and continue to supply solid instruction even to a beggarly number of students? Mr. Ryland, the majority of the Council, and myself agreed in choosing the latter alternative, but in spite of this it was evident that the Institution had arrived at a very critical stage of its existence, and my anxiety was considerable, having been so continually warned by "practical" men that "this sort of thing" had been tried again and again in Birmingham, and had always failed in the long run, however promisingly it may have begun.

The time had now arrived for proposing a scheme that I had been considering for some time past, and, accordingly, on one of my Sunday afternoon visits to Mr. Ryland, I laid before him the project of superadding to our regular courses of solid instruction occasional single lectures of a very popular introductory character, which anybody, however ignorant, might attend, and thus possibly be cured of the common mental epidemic of supposing that science is necessarily dry and repulsive, and ultimately be tempted to become students in the classes.

Mr. Ryland cordially approved, and we anxiously discussed the question of free admission, or a small charge of twopence, or threepence, or one penny at the doors, and finally decided on ONE PENNY. Mr. Ryland suggested the name, PENNY LECTURES, then a new one, as "Penny Readings" were yet unknown. He laid the project before the Council. It was adopted, and on Jan. 22, 1856, I commenced the first course of twelve penny lectures, the printed prospectus of which is now before me.

It was a general outline of natural phenomena, taking in what is now commonly taught as "Physiography." The success of the experiment was complete; the theatre was filled—at some of the lectures not only the seats were all occupied, but the platform was covered with a standing audience. The course was repeated "by special desire," and thenceforth, until I left Birmingham in 1864, I continued them every Tuesday during the session, with an occasional break, when Mr. George Dawson, or others, volunteered to occupy an evening. The subjects were various, in many cases determined by the limited stock of apparatus then within my reach, as, without ample demonstration, success was impossible. My object was to make the lectures as attractive and as *unsatisfactory* as possible, to awaken curiosity, but not to satisfy it there.

They had the desired effect. All classes of people, and of all ages attended them, the little boys in the front row being especially conspicuous. The classes steadily improved, instead of declining, as heretofore, and I am able to prove how they were fed by the statements of some of my best pupils, who told me how they were tempted to have a pen'orth on passing, then another, then to go every Tuesday, and finally to become what I then found them to be. If space permitted I could state some interesting personal details, among others, of the case of two notorious Birmingham burglars who regularly attended the Penny Lectures until the force of professional circumstances terminated their studies; of other Penny Lecture auditors who now stand well as scientific and technological experts and teachers of science; and how, through Isaac Smith, one of my most enthusiastic penny and class pupils, his uncle, Sir Josiah Mason, was induced to become the munificent founder of the "Mason College," which is doing for those who can afford to study during the day, what the Institute does for evening pupils.

*Penny Readings* followed the Penny Lectures, as the first initiation of the Literary Classes, and the highly successful Penny Arithmetic Lessons, by Mr. Rickard, revived the mathematical department, which, at about the date of the first Penny Lectures, had declined so seriously that an attendance of three, two, and even only one pupil was not uncommon in the Algebra Class. The present prosperity of the Institute is largely due to the remarkable energy and ability of Mr. Rickard, who proves himself a true teacher by throwing as much effort, earnestness, and enthusiasm into the simplification of simple addition as in the exposition of quaternions or the differential calculus.

When I left Birmingham the Council wisely appointed as my successor, both in the classes and the penny lectures, Mr. C. J. Woodward, who had proved the value of the Institute by first hearing a penny lecture, then entering the classes and gaining the Society of Arts gold medal while yet very young. He still works with untiring zeal and ability, and that simple-minded love of his work that is so contagious to pupils. His extended chemical laboratory and new physical laboratory are so commodious that they rival those of our great universities. A staff of able teachers has followed, and are now working admirably. In the general glorification of founders, patrons, &c., which justly occupied the recent meeting, there was a serious omission, viz., to do honour to the labours of such men as W. Matthews, George Dawson, Sam. Timmins, Dr. Badham, and others, who worked as unpaid volunteers in the pioneer labour of teaching classes when the Institute was too poor to pay professional teachers. The Institute now counts 2,688 students in the industrial department, against the 85 with whom I commenced, and 2,564 members in the general department. It has branches and affiliated institutions in every important suburb of Birmingham. Besides these, there is an "Institute Scientific Society," where original papers are read and discussed, and possessing a good lending library of scientific books; also a "Union of Institute Teachers and Students," for promoting the welfare of the Industrial department.

These, with the unrivalled Board schools, the King Edward's school and its branches, the great free library and public reading rooms and the Mason College, are effecting a wonderful transformation of the hardware metropolis, which will probably render it the most intellectual town in England.

I must not touch upon what may be termed the external history of the Institute, the visit of the great and good Prince Consort to lay the foundation stone of the building, and the many other public incidents connected with its growth; these will be duly recorded in "The Institute Book of Glory," promised by the hon. secretary; but I cannot conclude without a word or two respecting the analogy before-named between the work of the Midland Institute and that of KNOWLEDGE.

If Mr. Proctor were just emerging from Cambridge in the full-blown pride of newly-acquired Academical honours, I should despair of the success of this important effort to diffuse scientific knowledge among all classes of men, women, and children; but learning by the prospectus lately placed in my hands that he has been taking lessons from his own pupils, I have no doubt that he will successfully apply them.

Had I commenced as an Academical purist, with a cut-and-dried course of instruction framed on academical lines, the first classes of the Birmingham and Midland Institute would have failed, with serious consequences to the whole project. In like manner any attempt to popularise science by presenting to non-technical readers mere technical abstracts of technical papers or essays, must of necessity fail again, as it has failed before.

As an example, I may mention the fact that in my first Physics class not one of about forty students, mostly adults, knew what was a parallelogram or a diagonal. How, then, could I teach them the composition and resolution of forces? Such questions I had to ask and solve continually, and though I have not seen Mr. Proctor since his return from the Antipodes, I have no doubt that, during his lecture tours, similar questions have been thrust upon him, and that he has found answers for them, and will proceed accordingly.

The translation of the Latin word "Science" into plain Saxon "Knowledge" is a good beginning, and as Science, thus translated, is the natural food of the intellect of all human beings, there can be little doubt of the success of a well-conducted, non-pedantic (or, I would rather say, anti-pedantic) effort to supply the natural demand.

XUM

## COLOUR OF SUNLIGHT.

PROFESSOR LANGLEY threw out, several years ago, the idea that our sun is in reality not a white sun, but a blue one, his apparent whiteness being due chiefly to the absorptive action of his own atmosphere, but partly to that of our own air. It should be noticed, in considering this view, that our sun is probably akin to the stars which we regard as yellowish. Capella, for instance, is decidedly yellow by comparison with Vega. The spectrum of Capella has been shown by Dr. Henry Draper to be so similar to that of the sun that the eye can detect no difference. Arcturus, which is somewhat more markedly yellow—and, indeed, is regarded by some as orange-yellow in colour—has a spectrum very closely resembling that of the sun, but still not exactly the same. The great value of Dr. Draper's evidence on this point, like that of his evidence respecting the presence of oxygen in the sun (as shown by the comparison of the solar spectrum with that of our own air), is that it can be studied by all who choose to examine the photographs which he has obtained of the spectra for comparison. On the same small piece of glass are shown the spectrum of Capella and the spectrum of sunlight reflected from the surface of a planet, and the identity of the lines belonging to the two spectra can be seen at once. Now, Professor Langley, pursuing the idea which he threw out several years since, has been able to show that our sun, though we regard him as a white sun, and though, seen from a greater distance, so that he appeared as a star, he would probably appear yellowish, is yet a blue, or at least a bluish sun. We see that the sun appears red, or orange, or yellow (according to the state of the air) when setting—that is, when seen through a long range of our own atmosphere. Such action as our air exerts on the sun when he is high above the horizon is similar in character, though less in degree; it serves, therefore, *pro tanto*, to render the disc of the sun yellower than it really is. Again, the edge of the sun's disc is markedly less luminous than the middle, though to ordinary eyesight the difference is generally rendered undiscernible by the sun's great lustre. Of course, in reality the same amount of light is emitted from that part of the solar surface which at any moment lies near the edge of his visible disc as from that part which lies near the centre. The apparent difference can only be due to the effect of absorption exerted by the solar atmosphere—the line of sight passing through a greater range of this atmosphere when directed toward the edge than when directed toward the middle of the disc. (In passing, I may note that the difference of absorptive action is proof, not, as Kirchoff erroneously supposed, of the depth, but of the relative shallowness of the solar atmosphere.) Now, anyone who studies a photograph of the solar disc—as, for instance, that excellent photograph by Rutherford, which forms the frontispiece of Schiller's German translation of "Le Soliel"—will have noticed that the darkening toward the edge is much more marked there than it is in the ordinary telescopic disc of the sun. It follows that those rays which produce the photographic image of the sun (chiefly the blue, indigo, and violet rays) are more affected by the absorption of the solar atmosphere than those which form the ordinary visual image of the sun. His atmosphere then acts more strongly to absorb the rays belonging to the blue end of the spectrum than the rays belonging to the red end. The sun's apparent colour, therefore, is less blue than his real colour. If his atmosphere were suddenly removed, he would change from his present white or whitish-yellow colour to bluish, or perhaps greenish, precisely as the setting sun, if the air between the eye and him were suddenly removed, would change from his apparently ruddy hue to the white colour of the overhead sun. In this way it has been, only by detailed experiments instead of by general reasoning, that Professor Langley has established the theory that our sun is really blue, his apparent colour being due to the effect of absorption exerted by his atmosphere of vapours, chiefly metallic.

## THE *TRIBUNE* RIDDLE.

THE mystery of the *Tribune* Building* has long baffled the investigations of our most learned archæologists. Putting aside as unworthy of consideration certain wild theories of its purpose, there remain three leading theories, each one of which has its able advocates. By these we are told that the building was designed either for a tomb, or for religious purposes, or for an astronomical observatory.

The first of these rests on a comparatively slight foundation. The most elaborate research has failed to show that there is anything buried beneath the *Tribune* Building, except beer.

* The office of the New York *Tribune*.

The learned volume written by Prof. Veranda Brown in support of the theory that the *Tribune* Building represents the history of the Jewish and Christian religions, and contains a precious prophecy as to the future religious belief of mankind, has a certain degree of plausibility which will always commend itself to imaginative men. As is well known, the plan of the building is unlike that of any other architectural work, in the curious and arbitrary way in which it is divided into different stories. The lower part consists of four stories and a basement. According to Prof. Brown, this basement represents the patriarchal period before the time of Moses, and the four stories represent the Jewish nation under Moses, under the Prophets, under the Kings, and under the Roman Empire. The last story is smaller than the others, thus typifying the declining condition of the Jewish state, and the marble capitals of its pilasters indicate the luxury and splendour of the Roman Empire. Above the Jewish division of the building we find a second series of three stories, representing the first three centuries of the Christian era. The uppermost one of these is gorgeously ornamented with polished granite columns, with Byzantine capitals—constituting an unmistakable reference to the conversion of CONSTANTINE, the fixing of his capital at Byzantium, and the triumph of the Christian religion. Next we find three more stories, each containing five windows. These typify the fifteen centuries which have elapsed since the conversion of CONSTANTINE, and bring the record of the Jewish and Christian religions down to the present time.

Where the seventh story, or the third century of our era, begins, the façade of the *Tribune* Building is divided into two large masses, one being precisely twice the width of the other. These undoubtedly represent the division of the Christian world into the two great Churches—the Latin and the Greek—the beginning of which division really dates back to the rivalry between the Bishops of Rome and Constantinople. Between these two great divisions rises the tower—a quadrangular structure, with clear-cut angles, and standing boldly out into the light. In this tower we recognise Protestantism. Smaller in size than either of the two great divisions that represent the Latin and Greek Churches, it reaches nearer heaven, and secures a greater share of both sunlight and clouds.

The upper part of the tower is, in Prof. Veranda Brown's opinion, prophetic in its teaching. Both the tendency of Protestantism to bring all things to the bar of reason, and the refusal of any two Protestant bodies to agree in their views of what is true, are prefigured by the clock which occupies the upper part of the tower, with its four faces that so stubbornly refuse to make the same profession of faith as to the time. Still higher than the clock we find the lightning-rod. There the tower has reached its fullest development—ending in a point—signifying "nothing," and a lightning-rod, of all things the most thoroughly scientific and materialistic. Prof. Verandah Brown, who is a pure rationalist, accepts with some scientific enthusiasm this architectural prophecy of the fate of Protestantism.

The astronomical theory of the building is ably advocated by Prof. Pritchard Roctor. He maintains that had the builders intended to erect an astronomical observatory, they would have designed a building which should have been both massive and high. Both of these conditions are found in the *Tribune* building. They would also have constructed a long, narrow tube of masonry through which to observe the stars. We find precisely such a tube in the *Tribune* Building, although the ignorance of the present day has perverted it to the use of an elevator. They would also, says the Professor, have constructed a subterranean chamber as a receptacle for water. The *Tribune* builders builded even better than the Professor knew, for they made a large subterranean chamber for the reception—not of water, but of beer. From these considerations he draws the conclusion that the *Tribune* Building was designed for astronomical purposes, and incidentally remarks that the arguments of Professor Veranda Brown are fallacious, inasmuch as he forgot to include the coal-cellar in his calculations, and the coal-cellar cannot possibly be brought into any intelligible relation with the Jewish commonwealth.—*New York Times*.

ULTIMATE STRUCTURE OF BODIES.—"As matters now stand," says Mr. H. C. Sorby, "we are about as far from a knowledge (by vision) of the ultimate structure of organic bodies as we should be of a newspaper seen with the naked eye at a distance of one-third of a mile."

POND'S EXTRACT is a certain cure for Rheumatism and Gout.
Pond's Extract is a certain cure for Hæmorrhoids.
Pond's Extract is a certain cure for Neuralgic pains.
Pond's Extract will heal Burns and Wounds.
Pond's Extract will cure Sprains and Bruises.
Sold by all Chemists. Get the genuine. [ADVT.

## Our Mathematical Column.

MATHEMATICAL columns in magazines are too apt to degenerate into puzzledom corners, collections of problems of considerable difficulty, but having no real interest, and not valuable as illustrating principles. We wish ours to be of real use to the student of mathematics, but still more to those who, when studying other branches of science, find that rightly to understand their subject they require to be familiar with certain special departments of mathematics. It must be confessed that most of our treatises on mathematical subjects pay little attention to the requirements of students of this kind. They are excellently adapted to prepare students for examination; because at an examination a student of algebra must not be surprised if he be asked questions relating to infinite series, a student of trigonometry if he be asked to establish Dr. Moivre's Theorem, and so forth. But they do not meet the wants of one who requires to know the methods and principles of algebraical, trigonometrical, or other forms of calculation. There is little in such works to show the *use and value* of the processes considered in them. Many a ready student has passed a fair examination in the differential calculus, who had not the remotest idea of the practical value of its methods, or that in hundreds of simple subjects of inquiry the calculus can be employed easily and advantageously. There is no occasion for mystery respecting the use of mathematical methods; but it would really seem, to read some mathematical treatises, as though the last idea in the world the student should associate with any mathematical subject was the idea that it may actually be useful. Even the problems which are given for solution are, for the most part, either useless or absurd.

This is not the way to render mathematical subjects inviting, or to encourage the student to master the difficulties which are inherent in them.

Of course, we cannot here give mathematical essays which can render the student independent of systematic treatises. Such treatises he must have, and must carefully study. But we hope to show that a number of departments of mathematical research, supposed to be either too profound, or of too little practical value to be worth taking up, can be readily and usefully studied. In every case we wish to come as quickly as possible to the practical application of the various methods we shall successively deal with.

We shall probably begin with a simple paper on the use of logarithms, seeking to show that, instead of being regarded as a mere mysterious collection of numerals, a book of logarithms should be considered the great simplifier of all forms of calculation. Scarcely anyone who has much to deal with figures, otherwise than in mere processes of addition or subtraction, should be without such a book; and a very brief study of the subject will suffice to enable anyone to make ready and intelligent use of the tables which a book of logarithms contains.

We may then discuss the Laws of Chance, the simple applications of Trigonometry, and other such matters, avoiding, as far as possible, those parts of a subject which a student can readily study in set treatises.

We shall be glad to receive problems of interest, either for solution or discussion, preferring, however, those which illustrate general principles to those which are merely, as it were, casual.

In many cases where problems are sent to us for solution, we shall only give hints, not complete solutions, believing that, so dealt with, they are likely to be of more use to the student than if a complete solution were at once given.

We need hardly say that this is not a suitable place for the discussion of very difficult mathematical problems, albeit those of our readers who do not take interest in mathematics must not be angry with us if from time to time we devote a column, or even a page or two, to matters of no interest, or even absolutely unintelligible to them. They must remember that each of our lighter essays here may be equally interesting to proficients in the subject dealt with; that, in fact, no one can expect *all* the contents of such a journal as the present to be interesting to himself individually. We shall endeavour, however, to keep within close limits all matter likely to be "caviare to the general."

---

SUNLIGHT AND HEAT.—The intensity of sunlight at the sun's surface is calculated to be 190,000 times that of a candle-flame; 5,300 times that of the glowing metal in a Bessemer converter; 146 times that of the limelight; 3·4 times that of the electric arc at the brightest yet obtained. The heat emitted by the sun in a single second would suffice to melt a shell of ice covering the entire surface of the earth to a depth of 1 mile 1,457 yards, or to burn a layer of anthracite coal 17·7 yards thick over the earth's entire surface. This would be equivalent to a consumption of about 16,436 millions of millions of tons of such coal per second.

## Our Chess Column.

MANY weekly and monthly periodicals in this country and in others now contain a chess column, but it may be a question how far any general knowledge of the game is encouraged in this way. Usually these chess contributions are limited to problems, games between first-class proficients, and replies to queries, with occasional items of chess news. The problems are such as only good players can readily solve, so that the learner, unable to master them without an expenditure of time which he can ill spare, is rather deterred from the study of chess than otherwise by these masterpieces of chess strategy. The games, again, even when they are sufficiently annotated, are generally too profound to have much interest for the learner. He cannot see the purpose of moves whose real significance depends generally on results five or six moves at least in advance, and almost always on moves which are not played on either side. The chess expert recognises the beautiful positions, the brilliant attacks, the complex defences, and so forth, which would result if a particular move were not met in that way in which, in the game itself, it actually is met. A volume would be required to show all such results of each move played on either side by two first-class players; and the beauty of a game between two such players can only be properly appreciated by those who, as each move is played, can follow out the various consequences, according to the way in which the move may be met.

We wish to cater for a wider circle of chess players—for those who enjoy the game and can appreciate its beauties, while they have no ambition to be able to meet a Steinitz, a Blackburne, or a Zukertort without receiving odds.

It must be admitted that nine out of ten who call themselves chess-players know very little about the game. They move their pieces without any adequate idea of the value of position, or of the manner in which the powers of the various pieces can best be brought out. Of the importance of time, again, in chess strategy, they seem to have scarcely any conception. A player of this class will move his Queen out to some square where she can be attacked by a piece which in attacking her will take up a strong position, then to some other square where she can be similarly attacked by another piece, and, perhaps, after four or five such moves, be fortunate in being able to return her to her own square. He will then complacently remark that he must try some other way of opening his attack, utterly overlooking the fact that his opponent has gained four or five moves, and that even a single move early in the game often makes all the difference between a strong attack and an unsatisfactory defence. If such a player moves his Knight from King's Bishop's third to King's Knight's fifth, and on the opponent moving Pawn to King's Rook's third (attacking the Knight), finds no better move than to return him to his former position, he would be incredulous if told that he had thus lost all chance of winning against correct play. Yet there is scarcely an imaginable case, in the early stage of a game, where, if the game had been equal before this had happened, it would not afterwards have been seriously compromised. The opponent, be it observed, has made a move of great utility (though often too slow, except as in this case when a move is given away), while it is now his turn to play instead of the first player's, who stands just where he did before he rashly moved forth his Knight. (The opening, of which the accompanying game is an illustration, shows that, even when by venturing forth the Knight to the square in question a Pawn is gained, the counter attack, after the Knight has been driven back, compensates the second player fully for the loss. It also presents at move 33, a case in which loss of time is equivalent to loss of a game which might probably have been drawn.)

It seems to us that we may do something to encourage sound chess play by giving our readers chess material of a different kind from that which has usually been presented in chess columns. We propose to explain in a series of short papers the principal openings, discussing their advantages for attack and defence, and showing how the opening moves illustrate the general principles on which sound play depends. We shall illustrate these openings by games played by good players, but not played with quite so much study (and, therefore, not needing such skill for their interpretation) as the match games commonly selected for these columns. For this purpose we have made arrangements with the proprietor of the mechanical chess-player, Mephisto, to have games played with Mephisto specially for our chess columns. By an extension of Mr. Gümpel's kindness, the guiding spirit of that mysterious player has been persuaded to make his own comments on the game. It need hardly be said that only such games will be selected as have real chess interest. The multitudinous contests in which tyros have succumbed to Mephisto (and would have succeeded at the odds of a Queen) would be quite as much out of place here as in the

XUM

columns of the *Chess Chronicle*, humbler though our aim may seem.

We shall assume on the part of our readers a knowledge of the elements of chess play—the moves, laws, and so forth; and sufficient familiarity with the notation employed by English-speaking players, according to which the King (K.), the Queen (Q.), the Bishops (K.B. and Q.B.), the Knights (K.Kn. and Q.Kn.), and the Rooks (K.R. and Q.R.) are regarded as standing severally on the first square of a file, the other squares of which are numbered in order, the 2nd, 3rd, 4th, 5th, 6th, 7th, and 8th.

For our chess readers' amusement we give an illustration of weak chess play, which occurred within the last few days over our own chess-board. The first player had the idea that, knowing little of the usual openings, he might equalise matters by playing on a line entirely unrecognised by the books:—

### CHESSIKIN.

*Remove Black's Q.R.*

| White. Amateur. | Black. Chess Editor. |
|---|---|
| 1. P. to K.4. | P. to K.4. |
| 2. P. to K.R.4 (?) | Kt. to K.B.3. |
| 3. Kt. to Q.B.3. | B. to Q.B.4. |
| 4. R. to K.R.3. (?) | P. to Q.3. |
| 5. R. to Q.3. (?) | B. to K.Kt.5. |
| 6. P. to K.B.3. | Kt. to K.R.4. |
| 7. Q.Kt. to K.2. (*) | Q. takes P. (ch.). |
| 8. P. to K.Kt. 3. | Kt. takes P. |
| 9. Kt. takes Kt. | Q. takes Kt. (ch.). |
| 10. K. to K.2. | Q. to K.B.7. mate. |

* The game was still defensible, by—

7. $\frac{\text{P. take B.}}{\text{Q. to K.R.5. (ch.).}}$ 8. $\frac{\text{P. to K.Kt.3.}}{\text{Q.Kt. takes P.}}$ 9. $\frac{\text{Q. to K.B.3}}{\text{B. take Kt.}}$ &c.

### GAME No. 1.

Played between the Mechanical Chessplayer, "Mephisto," and an Amateur.

TWO KNIGHTS' DEFENCE.

| White. Amateur. | Black. Mephisto. |
|---|---|
| 1. P. to K.4. | P. to K.4. |
| 2. Kt. to K.B.3. | Kt. to Q.B.3. |
| 3. B. to B.4. | Kt. to B.3 (a). |
| 4. Kt. to Kt.5. | P. to Q.4. |
| 5. P. takes P. | Kt. to Q.R.4. |
| 6. P. to Q.3 (b). | P. to K.R.3. |
| 7. Kt. to K.B.3. | P. to K.5. |
| 8. Q. to K.2. | Kt. takes B. |
| 9. P. takes Kt. | B. to Q.B.4. |
| 10. P. to B.3 (c). | B. to K.Kt.5 (d). |
| 11. P. to K.R.3. | B. takes Kt. (e). |
| 12. P. takes B. | Castles. |
| 13. P. to Kt.4. | R. to K.sq. (f). |
| 14. P. to B.4. | B. to Q.3. |
| 15. B. to K.3. (g). | P. to Q.R.4. |
| 16. P. takes P. (h). | R. takes P. |
| 17. Kt. to Q.3. | R. to Q.R.6. |
| 18. Kt. to Kt.3. | Q. to R.sq. (i). |
| 19. Castles K.R. | Q. to B.sq. (j). |
| 20. K. to R.2. | Q. to B.4. |
| 21. Q. to Q.2. | Kt. to R.4. |
| 22. Kt. to Q.4. | B. takes P. (ch.) |
| 23. B. takes B. | Q. takes B. (ch.) |
| 24. Q. takes Q. | Kt. takes Q. |
| 25. Kt. to Kt.5. | R. to R.5. |
| 26. Kt. takes P. | R. to Q.B.sq. |
| 27. P. to Q.6. | R. takes B.P. |
| 28. K.R. to K.sq. | Kt. to Q.6. |
| 29. R. to K.2. | R. to Q.sq. |
| 30. Kt. to Kt.5. | P. to B.4. |
| 31. P. to B.3. (k) | P. takes P. |
| 32. R. to K.7. | Kt. to B.4. |
| 33. R. to K.Kt. sq. (l) | P. to Kt.4. |
| 34. R. to K.B. sq. | R. to R.5. (m) |
| 35. P. to R.3. | P. to Kt.5. |
| 36. P. takes P. | P. takes P. |
| 37. R. from B. sq. to K. sq. | P. to B.7. |
| 38. R. to K.8. (ch.) (n) | R. takes R. |
| 39. R. takes R. (ch.) | K. to B.2. |
| 40. R. to K.7. (ch.) | K. to B. sq. |
| 41. K. to Kt.2. | R. to K.B.5. |
| 42. K. to B.sq. | P. to Kt.6. |
| Resigns (o) | |

### MEPHISTO'S NOTES.

(a) This move constitutes the Two Knights' Defence.

(b) This move gives White if not a bad, at least a difficult, game to play; the continuation 6.B. to Kt.5. (ch.) is to be preferred.

(c) This move is stronger than the usual move P. to K.R.3. White threatens an attack with his Pawns on the hostile Bishop, thereby developing also his strong Queen's wing. P. to B.3. also provides a refuge for White's Knight on Q.4. in case Black should Castle, which would leave the Knight *en prise*.

(d) This certainly seems the most attacking line of play, in addition to which Black could also play P. to Q.R.4. to prevent the advance of the Queen's Pawns or P. to Q.Kt.4., or Castles.

(e) This is better than B. to R.4., which would result to the advantage of White, *e.g.* 11. $\frac{}{\text{B. to R.4.}}$ 12. $\frac{\text{P. to K.Kt.4.}}{\text{B. to Kt.3.}}$ 13. $\frac{\text{Kt. to K.5.}}{}$ with the better game.

(f) Threatening to win the Queen.

(g) This is the right move to stop any advance of the Black King's Pawn. Thus, for instance, 15. $\frac{\text{P. to Q.B.5}}{}$ instead, would not be good; for 15. $\frac{\text{P. to Q.B.5}}{\text{P. to K.6}}$ 16. $\frac{\text{B. takes P.}}{\text{B. takes K.B.P.}}$ with the better game. White could not take the Bishop, for then Black would win his Queen by P. takes P. (ch.).

(h) Black has played P. to Q.R.4, with the intention of breaking up the Pawns on White's Queen's wing. If instead of P. takes P., as actually occurred, White should play P. to Kt.5, then P. to Kt.3 would stop White's Queen's Pawns. Black might, perhaps, also reply with P. to B.3. The variations arising out of this move are very numerous. The idea is the same as in 15. $\frac{}{\text{P. to Q.R.4,}}$ namely to separate White's Pawns, and then attack them singly, *e.g.*:—

16. $\frac{\text{P. to Kt. 5}}{\text{P. to B. 3}}$ 17. $\frac{\text{P. to B.5 (or see A)}}{\text{B. to Kt. sq.}}$ 18. $\frac{\text{P. to K.6}}{\text{P. takes P.}}$

19. $\frac{\text{Q. takes P.}}{\text{Q. to Q.2}}$ 20. $\frac{\text{Q. takes Q.}}{\text{Kt. takes Q.}}$ 21. $\frac{\text{Kt. to Q.2}}{\text{B. to R.2}}$ 22. $\frac{\text{Kt. to Kt. 3}}{\text{P. to R.5}}$ with the better game.

A. It would be disadvantageous to take with the Queen's Pawn, thereby opening the file commanded by the Black Queen; therefore, 17. $\frac{\text{Kt.P. takes P.}}{\text{P. takes P.}}$ If now White should play P. takes P., then Q. to B.2 would give Black a very good game, or if White should play, 18. $\frac{\text{P. to B.5}}{\text{B. to Kt.sq.}}$ 19. $\frac{\text{P. to Q.6}}{\text{B. to Kt.2}}$ 20. $\frac{\text{Kt. to Q.2}}{\text{Q. to B.sq.}}$ with the object of playing Q. to B.4, followed, perhaps, by Kt. to Q.2., Black should again get a good game.

(i) Threatening the capture of the Knight.

(j) By 19 Castles, White thought to evade Black's attack on the Queen's side, but only to exchange it for an attack on the King's side, the chance of which Black at once follows up by Q. to B.sq.

(k) White hopes to be compensated by his attack of R. to K.7 for this move.

(l) This is loss of time, as Black thereby advances his Pawns in support of the Bishop's Pawn. White cannot venture upon anything for fear of P. to B.7; if K. to K.3, then Black wins the White King's Pawn by Kt. to K.5 (ch.)! therefore, 33 R. to K.B.sq. at once was White's best play.

(m) This is hardly necessary; the object was to make the Rook available for support of the other Rook *viâ* R. to R. sq.

(n) This is weak again. R. to B.7 would have given White a good game; for, in reply to $\frac{}{\text{R. to B.4,}}$ White would play K. to Kt.3 with a fair chance of drawing.

(o) Black threatens to Queen his Pawn by P. to Kt.7 (ch.), which White cannot prevent.

---

KNOWLEDGE AND IGNORANCE.

"Ignorance is the curse of God,
Knowledge the wing on which we fly to Heaven."
—*Shakespeare.*

---

*** WHIST COLUMN.—Our papers on Whist will be commenced in No. 2.